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FOREWORD

The Environmental Protection Agency, through the Industrial Environmental Research Laboratory, Research Triangle Park, N. C. (IERL-RTP) sponsored a seminar on In-Stack Particle Sizing for Particulate Control Device Evaluations. The seminar was organized by the Process Measurements Branch and held at IERL-RTP on December 3 and 4, 1975.

The seminar was chaired on December 3 by D. B. Harris, and opening remarks were presented by J. A. Dorsey, Chief, Process Measurements Branch, IERL-RTP. Nine technical papers were presented, followed by a general discussion of in-stack particle sizing.

The December 4 session was chaired by L. E. Sparks. Seven technical papers were presented, as well as a panel discussion of in-stack use of diffusion batteries and further general discussion. Questions were asked and answered during and following each presentation.

This document is composed of edited versions of the speakers' transcripts from the seminar. Some were extensively edited into technical paper format, while others remained conversational in tone. Visual aid material presented by the speakers is included.

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OPENING REMARKS

J. A. Dorsey, Chief, Process Measurements Branch, IERL-RTP

HARRIS: We should get this program started. Jim, would you come up and give us a start?

DORSEY: I think that one reason for this seminar is to introduce the engineering people present to the individuals responsible for measurements. The engineering people have said to the measurements people, "I want to watch;" and quite often the measurements people have said, "Well, you can't." Then they have gone out to the source and made a series of tests with cascade impactors and diffusion batteries and have come back and told the engineers, "This is the particle size distribution." When the engineer says, "How do you know?" The response is, "Well, I know because I did it." And so, since we wouldn't let them watch, some of them said, "Let's get them all together and make them at least tell us what the devil they are doing out there."

I really think that ought to be the theme of this whole seminar. It is not to discuss all the things that we might do someday, but rather, what we are doing, and what's the meaning of what we're doing. Les [Sparks] has the opening paper, and I think that it should also be called an opening comment in that it sets the whole tone for this seminar by answering the questions: What are we getting impactor data for? What are we going to use it for? There is a great deal of relevance to that in that most of us here are not getting it simply because we want to publish a paper on how you use a cascade impactor, or what the wall losses are, or what happens if you have a substrate reaction problem. That is not the goal of the majority of people here. The goal is to get usable, useful, and, I think, definable (what does it mean?) size distribution data that is meaningful to the control device evaluator.

I don't want to take any more time with general comments. I will reemphasize the point that all during the seminar I hope everybody here continually asks--not what kind of data can I get, or what could I do to build a better impactor, or conduct a program that's the ultimate in making size distribution measurements--but rather, what do we need to do to evaluate control devices and are the measurement techniques, are the technologies, is the hardware we are using giving us the answers that we want? Is it giving the engineer what he is looking for?

In substance, my opening statement is: throughout this seminar, let's all continually ask and reiterate the question--is what I'm reporting on, is what I'm doing, relevant to what the engineer wants and what the engineer needs to evaluate a control device or to improve the operation of a control device?

USE OF PARTICLE SIZE DATA

L. E. Sparks, Particulate Technology Branch, IERL-RTP

HARRIS: Les Sparks will start us off. Les is really the guy that started this whole thing because he kept saying, "What are you telling me?" So he is going to tell you now what he needs to know, which he hasn't been able to do to me yet in three years.

SPARKS: Thank you Bruce.

Basically, in the Particulate Technology Branch, we're using cascade impactor data for two purposes--equipment design and device comparison.

We sponsor work on developing design methods for scrubbers, precipitators, and baghouses. Our design models are based on the assumption that you can predict the overall collection efficiency of a device, knowing the collection efficiency as a function of particle size and the particle size distribution. Now I feel that we can predict the collection efficiency of a given size particle, at least for scrubbers and precipitators. That is, we can predict the collection efficiency of a half micron particle under a given set of operating conditions in a scrubber and a precipitator.

Then the problem is integrating these individual particle penetrations over the particle size distribution, and here's where we get into problems: you have got to go out to the stack and stick a probe in there and get a size distribution that means something. And, unfortunately, particle control devices are extremely sensitive to particle size distribution, and that means the mass mean particle diameter and whatever other parameters it takes to characterize the size distribution. Unfortunately, I think a lot of stuff got into the literature that said mass mean diameter is all you worry about and geometric standard deviation and everything else do not really matter.

I have used the mathematical models that our contractors developed to make some estimates of what happens when size distributions change by 10 percent. All the calculations were based on log-normal particle size distributions. For a scrubber, a 10 percent change in the geometric standard deviation could mean a factor of 2 or more difference in pressure drop required for a given efficiency. For precipitators, the same change in geometric standard deviation could mean a factor of 30 percent difference in the specific collector area required to maintain a given collection efficiency. These are huge design differences. Thirty percent in a large specific collector area precipitator for power plants is millions of dollars; and a factor of 2 in pressure drop for a large source is a significant number of dollars per year of operating life. So that's the first reason why I worry about a few tenths of a milligram here and a few tenths of a milligram there on the stage weights.

The other use that we have for impactor data or size distribution data is for comparing devices. Is Device A a better device than Device B or is Operating Condition A a better operating condition than Condition B? And here again it's the same problem, control devices respond to the size distribution more than our measurements do. If you've got 30 percent errors and uncertainties in your penetration curves, which are probably reasonable to assume, then you need a dozen data points or so to find a factor of 2 difference in collector efficiency; and as everybody here who takes data knows, getting a lot of data is expensive. A large field test program costs a lot of money. I'd like you people to reduce the amount of money you need to do it.

It doesn't matter whether we try to do these device evaluations in the laboratory or in the field, we need good measurements. And if we do the evaluation in the laboratory, we need some kind of consistent aerosol generator--which introduces its own problems.

I guess another use for particle size distribution measurements is to define source variations. Can we find out when the source varies and what the source variation is? Based on the impactor data that were taken to characterize an aerosol generator recently, I wonder if the impactor is introducing more noise than the aerosol generator.

Whether we're using impactor data for equipment design or equipment comparison, we need to be able to tell people in our reports that the what-much-what scrubber is no darn good or it's only as good as every other

scrubber you can buy. Or whether doubling the size of a precipitator or going to a strange electrode configuration on a precipitator is really a good idea. And at the moment we are not able to do that very well because of the uncertainty in our size distribution measurements. I hope that, by the time everybody leaves here, they'll be able to go back and make those 1-percent errors in mass mean and 1-percent errors in geometric standard deviation size measurements.

CALIBRATION OF SEVERAL IN-STACK CASCADE IMPACTORS

Wallace B. Smith, Southern Research Institute

HARRIS: Now that everybody has a definition of the problem that we are trying to solve, we'll start into work on the impactor. For the last three years we have been contracting with Southern Research Institute to try to figure out what this wooly animal was doing to us. I keep hoping Wally's [Smith] going to be able to tell me exactly what it does, but every time he does, he says that it does it under this consideration and we have three other things that it's doing that we don't understand. The first year we sprayed them (impactors) out. We said, "Well, these things work nicely, so we're just going to go out and see how well they work in the field." After that disaster, we went back inside the laboratory to see how they work. So Wallace Smith of Southern Research is going to outline for us all the data that they got under well-controlled conditions in the laboratory, with Les' 1-percent accuracy.

SMITH: The procedures used to perform this calibration study were as follows: a fluorescent, monodisperse aerosol (ammonia fluorescein) was generated using a vibrating orifice aerosol generator (VOAG). The aerosol was sampled with each impactor at flow rates which are within the bounds specified by the manufacturer. The impactors were then disassembled completely and all the internal surfaces washed in separate, measured volumes of 0.1N NH_4OH. The mass accumulated on each surface was measured by absorption spectroscopy using a Spectronic 88 spectrometer. Thus, wall loss and stage collection efficiency data were obtained for all particle sizes tested. The calibration aerosols ranged from about 1 to 15 μm in diameter. It is difficult to produce smaller particles than 1 μm because nonvolatile impurities in the solvent contribute significant errors. The impactor flow rates were not varied during these tests. Also, it was necessary to sample for very long periods in order to obtain reliable data when smaller particles were used.

The substrates that were used were glass fiber, Vaseline, bare metal, and Teflon. The bare metal and Teflon were unsuitable, and experiments with these were soon discontinued.

This work was done under contract to the EPA, with Bruce Harris as the project officer. All these data and more will be included in our final report to him.

Each figure that is shown will identify the impactor, the individual stage, the jet velocity, the manufacturer's value for the stage D_{50}, and the Reynolds number for the jet.

The first impactor for which data are shown is the Andersen Stack Sampler, or Mark III. Gelman Type A glass fiber substrates were used, and the impactor was operated at a flow rate of 0.5 ACFM.

Figure 1 - Stage 1 - Andersen

The manufacturer's D_{50} is 12 μm is for our test conditions, and the data show that to be accurate.

Figure 2 - Stage 2 - Andersen

The manufacturer's D_{50} is 7.5 μm. Again that is very close. Notice however, that the stage efficiency peaks near 90 percent and falls off for larger particles. This is a form of nonideal behavior which has previously been observed by us and by others. One would prefer the efficiency curve to rise to 100 percent and maintain that value for all larger particles.

Figure 3 - Stage 3 - Andersen

The manufacturer's D_{50} for this stage is 5.1 μm. Figure 3 shows that the calibration data agree quite well with the predicted D_{50}. Jet velocity is 1.3 m/sec; the Reynolds number is 78. Again, the experimental efficiency curve for this stage rises sharply to a value between 80 and 90 percent, then drops to lower values for very large particles.

Figure 4 - Stage 4 - Andersen

This figure shows our calibration data for stage 4 in the Andersen impactor. The manufacturer's D_{50} is 3.4 μm, while our calibration data show a D_{50} of approximately 4 μm. This is still reasonably good agreement.

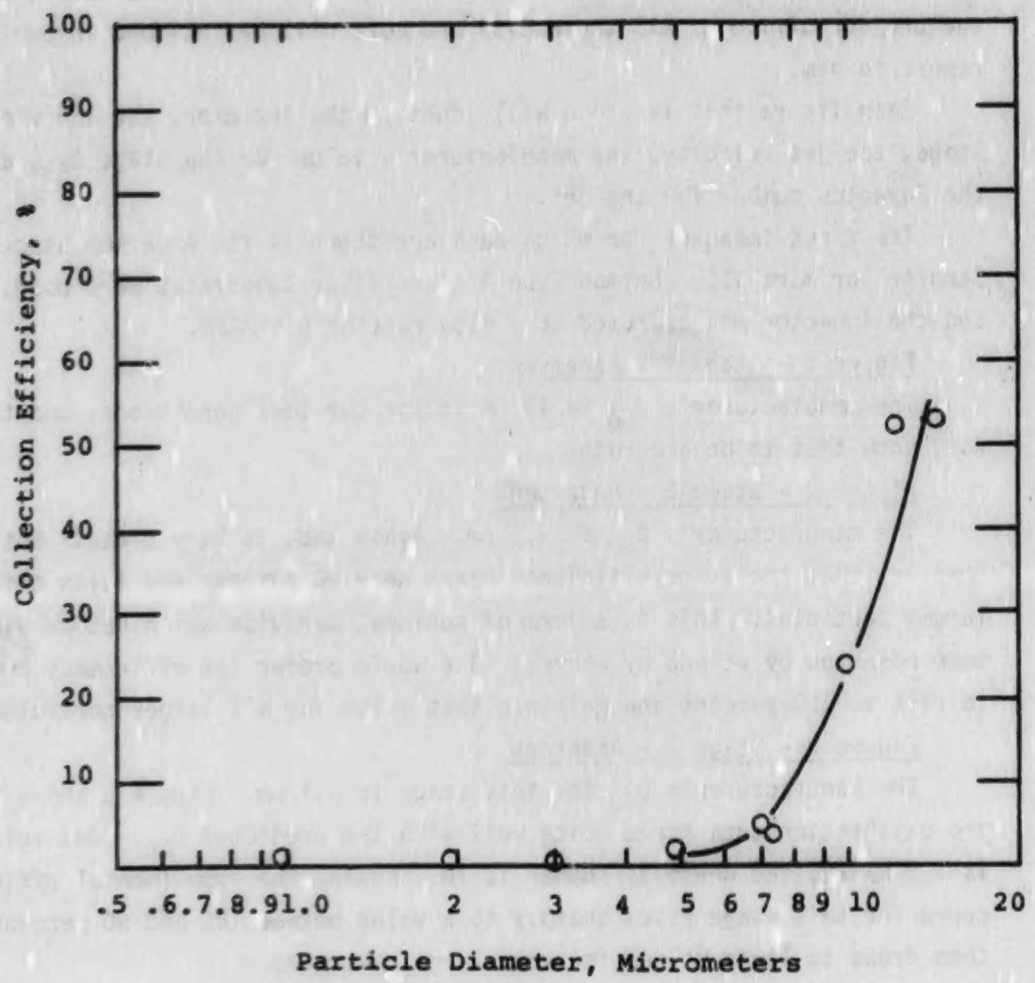


Figure 1. Andersen Impactor, glass fiber substrates, stage 1. V_j = 0.4 m/sec, Re = 45, calculated D_{50} = 12 µm.

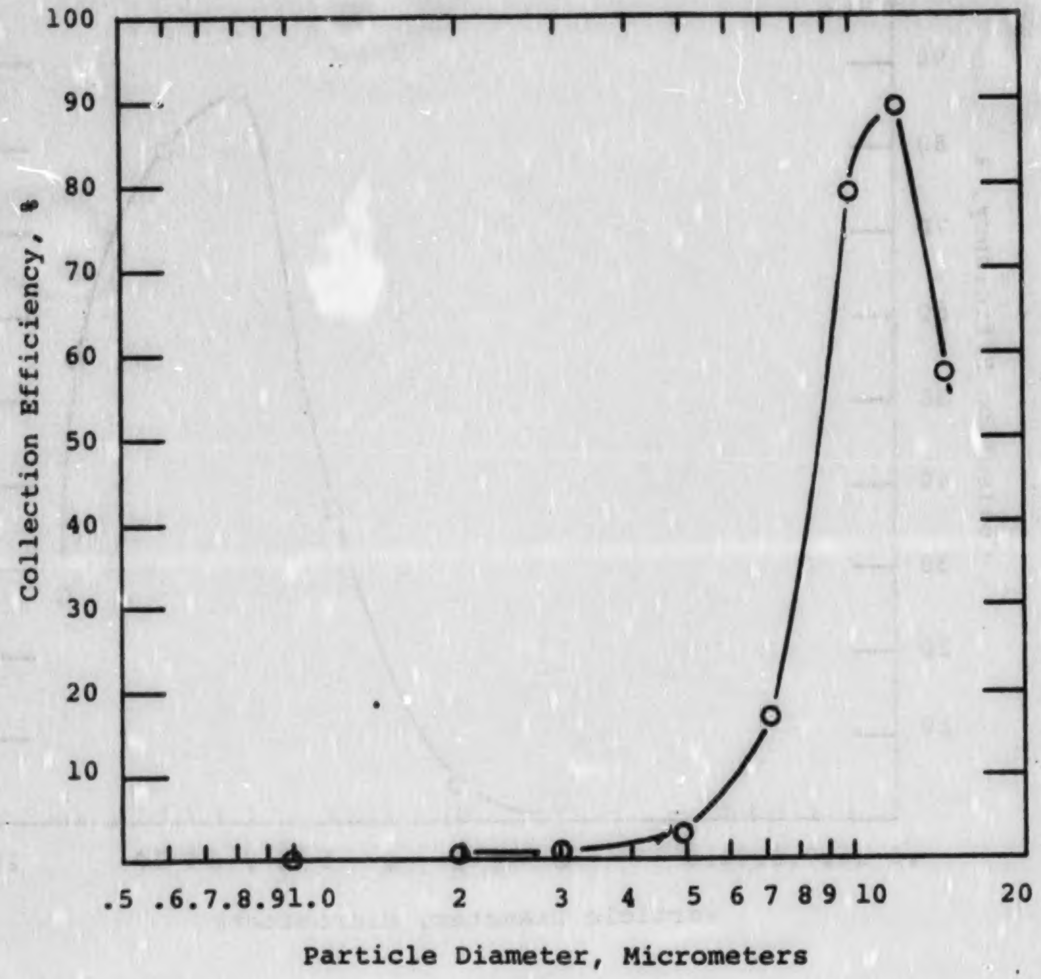


Figure 2. Andersen Impactor, glass fiber substrates, stage 2. V_j = 0.7 m/sec, Re = 57, calculated D_{50} = 7.5 μm.

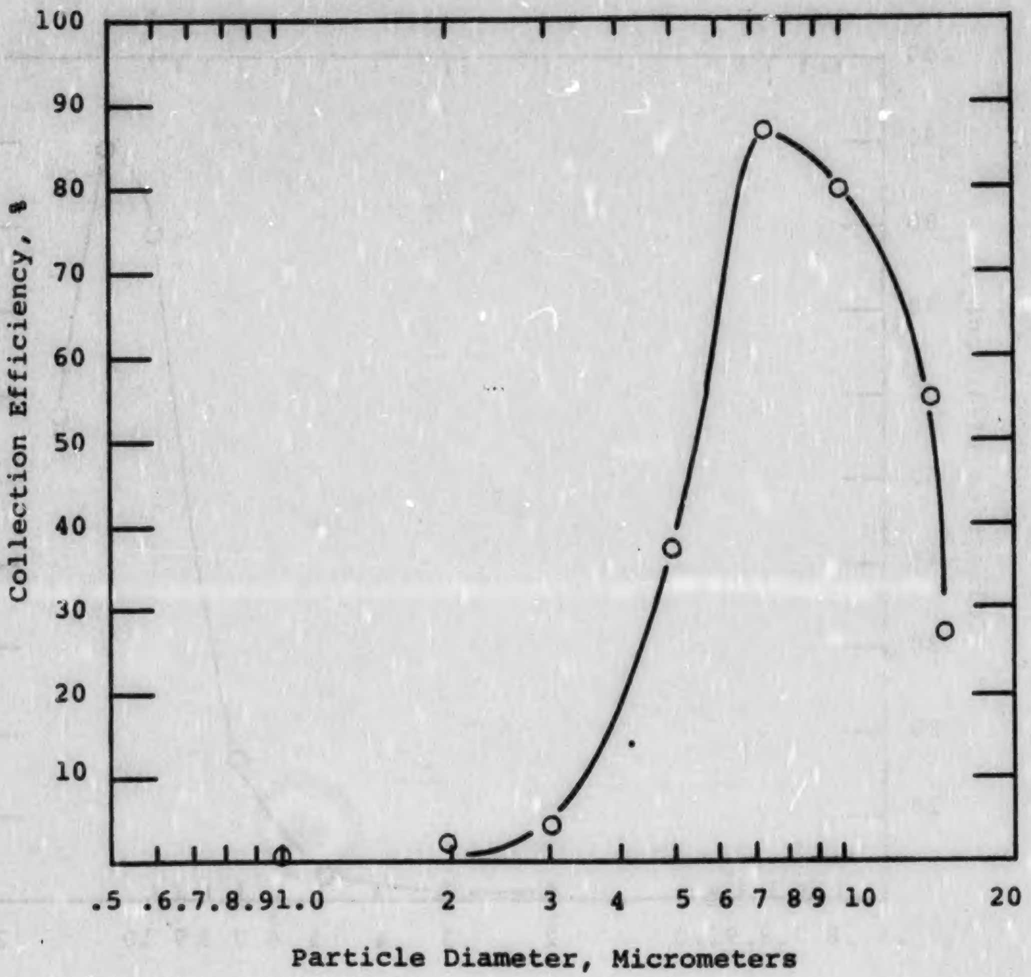


Figure 3. Andersen Impactor, glass fiber substrates, stage 3. V_j = 1.3 m/sec, Re = 78, calculated D_{50} = 5.1 μm.

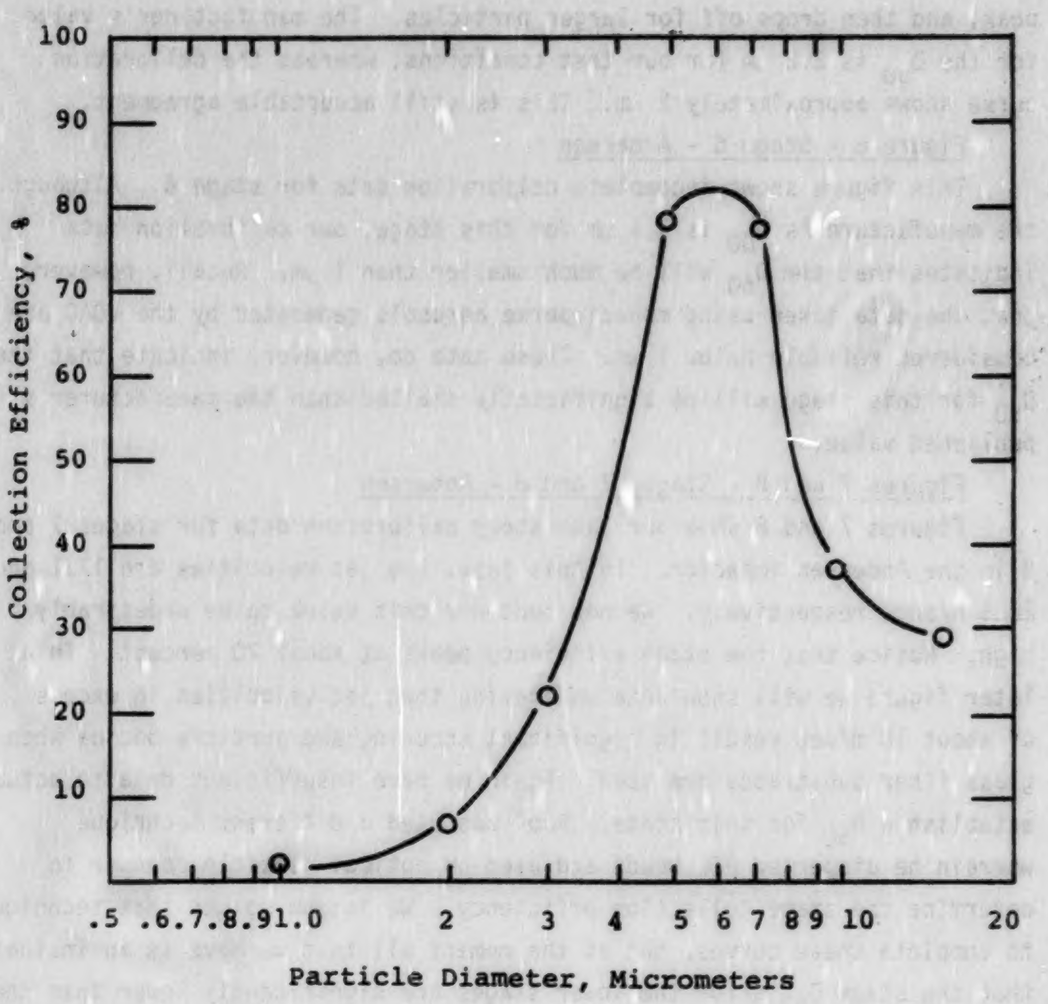


Figure 4. Andersen Impactor, glass fiber substrates, stage 4. V_j = 2.0 m/sec, Re = 99, calculated D_{50} = 3.4 μm.

Figure 5 - Stage 5 - Andersen

The calibration data shows that stage 5 performs very well. The calibration curve rises to approximately 90 percent with a fairly broad peak, and then drops off for larger particles. The manufacturer's value for the D_{50} is 2.2 μm for our test conditions, whereas the calibration curve shows approximately 2 μm. This is still acceptable agreement.

Figure 6 - Stage 6 - Andersen

This figure shows incomplete calibration data for stage 6. Although the manufacturer's D_{50} is 1.1 μm for this stage, our calibration data indicates that the D_{50} will be much smaller than 1 μm. Recall, however, that the data taken using monodisperse aerosols generated by the VOAG are not considered reliable below 1 μm. These data do, however, indicate that the D_{50} for this stage will be significantly smaller than the manufacturer's published value.

Figures 7 and 8 - Stages 7 and 8 - Andersen

Figures 7 and 8 show our laboratory calibration data for stages 7 and 8 in the Andersen impactor. In this case, the jet velocities are 17.1 and 28.5 m/sec, respectively. We now consider this value to be undesirably high. Notice that the stage efficiency peaks at about 70 percent. In a later figure we will show data indicating that jet velocities in excess of about 10 m/sec result in significant scouring and particle bounce when glass fiber substrates are used. Again we have insufficient data to actually establish a D_{50} for this stage. Rao[1] has used a different technique wherein he dispersed PSL beads and used an optical particle counter to determine the stage collection efficiency. We intend to use that technique to complete these curves, but at the moment all that we have is an indication that the stage D_{50}'s for the lower stages are significantly lower than the manufacturer's published values.

Figure 9 - Andersen Wall Losses

Wall losses were determined, as we stated earlier, by washing all the internal surfaces of the impactor and using the absorption spectroscopy technique to determine the amount of mass collected on each surface. Figure 9 shows the wall loss data for each location inside the impactor and as a function of particle size. The majority of the particulate lost inside

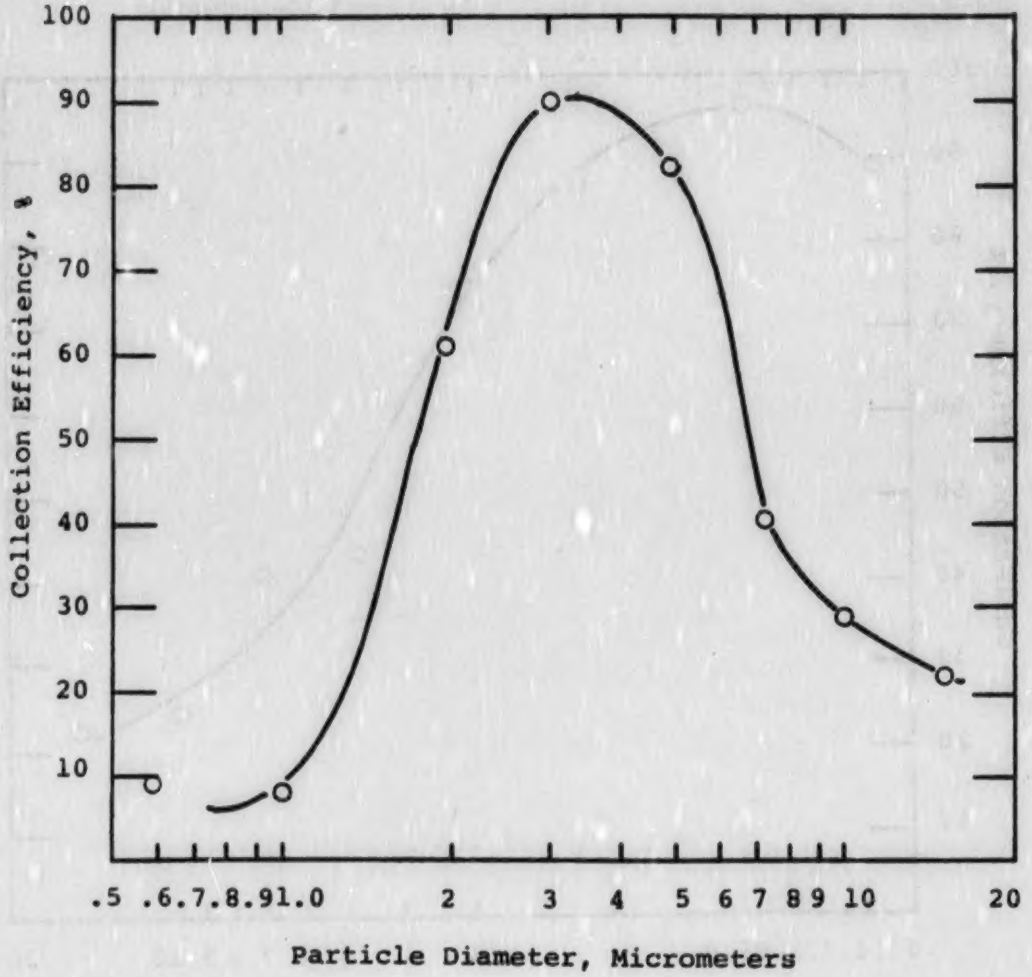


Figure 5. Andersen Impactor, glass fiber substrates, stage 5. V_j = 3.5 m/sec, Re = 130, calculated D_{50} = 2.2 μm.

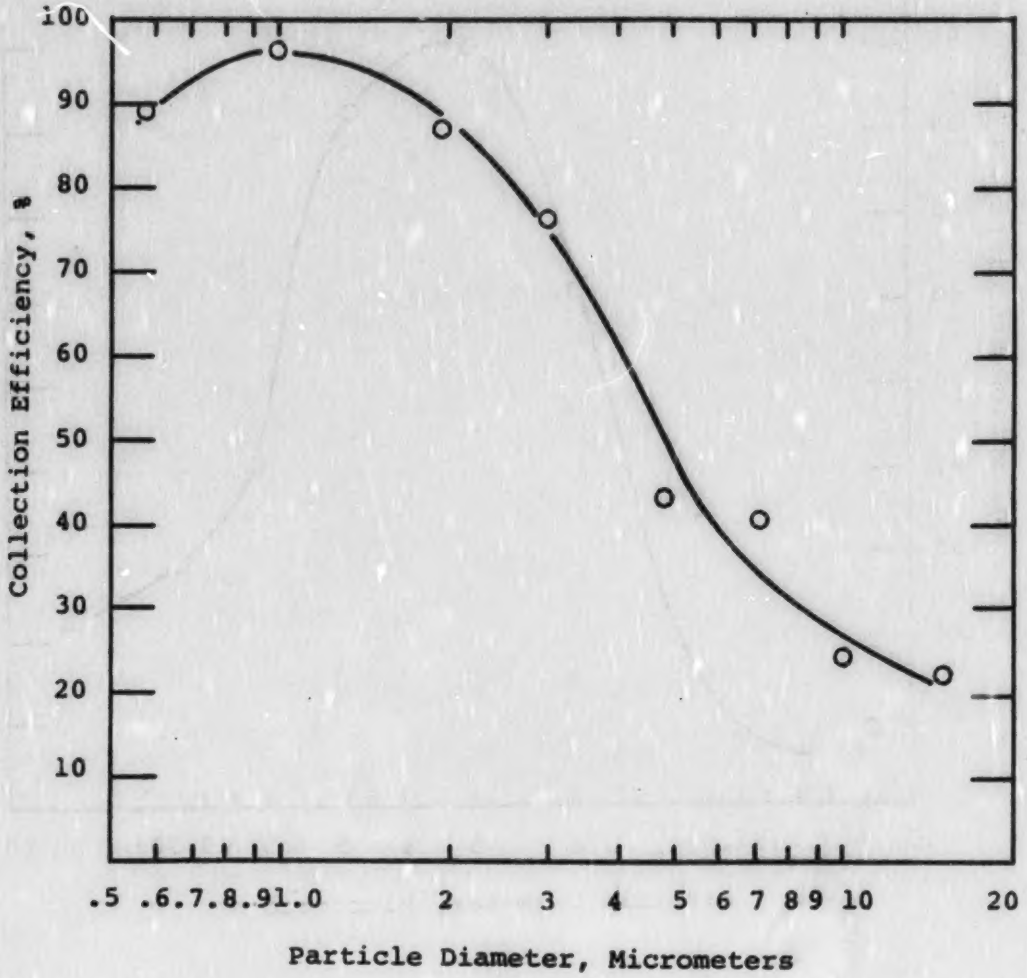


Figure 6. Andersen Impactor, glass fiber substrates, stage 6. V_j = 9.3 m/sec, Re = 210, calculated D_{50} = 1.1 μm.

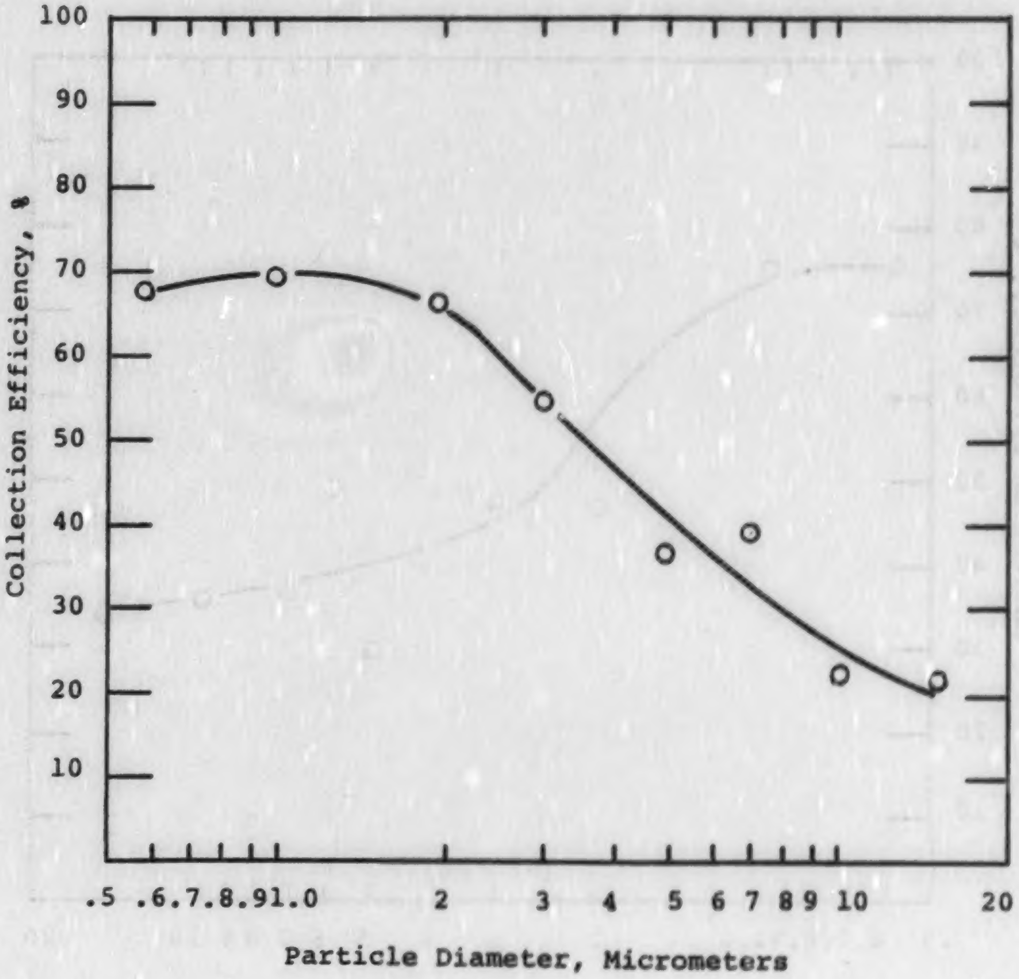


Figure 7. Andersen Impactor, glass fiber substrates, stage 7. V_j = 17.1 m/sec, Re = 284, calculated D_{50} = 0.69 μm.

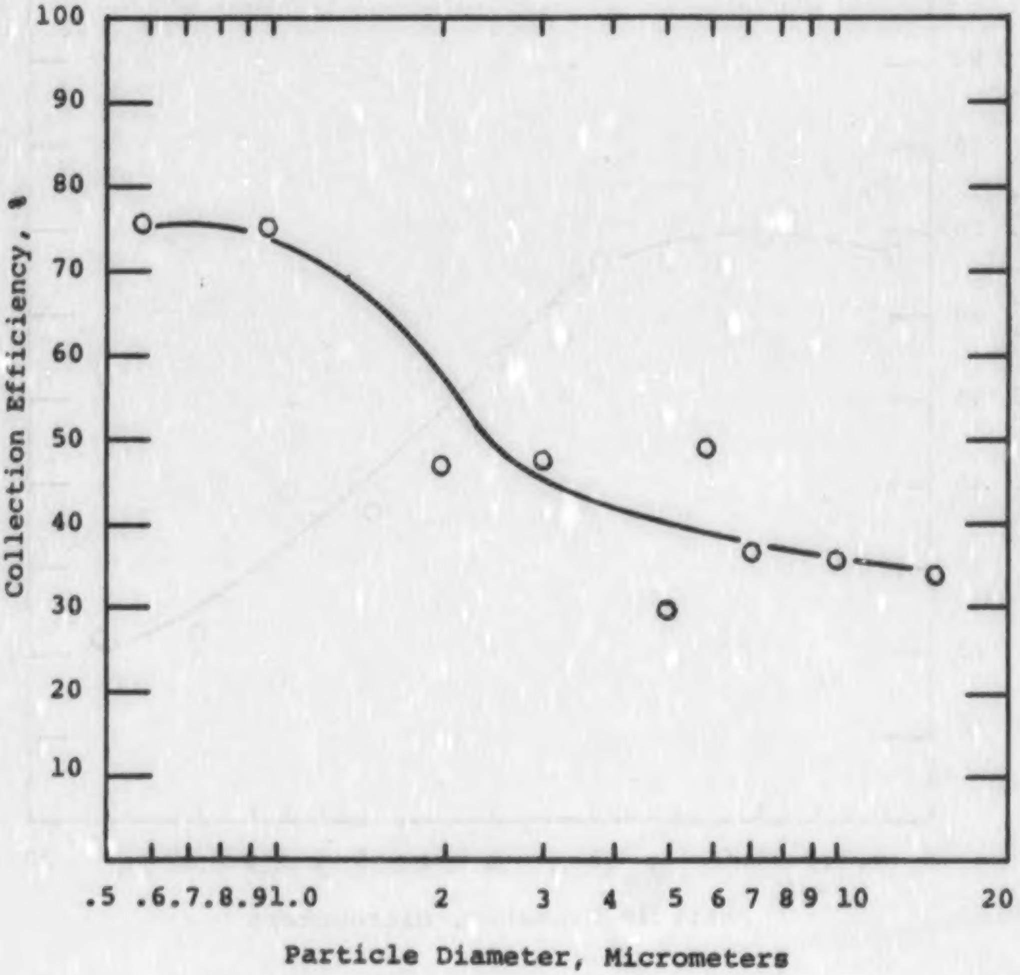


Figure 8. Andersen Impactor, glass fiber substrates, stage 8. V_j = 28.5 m/sec, Re = 475, calculated D_{50} = 0.44 μm.

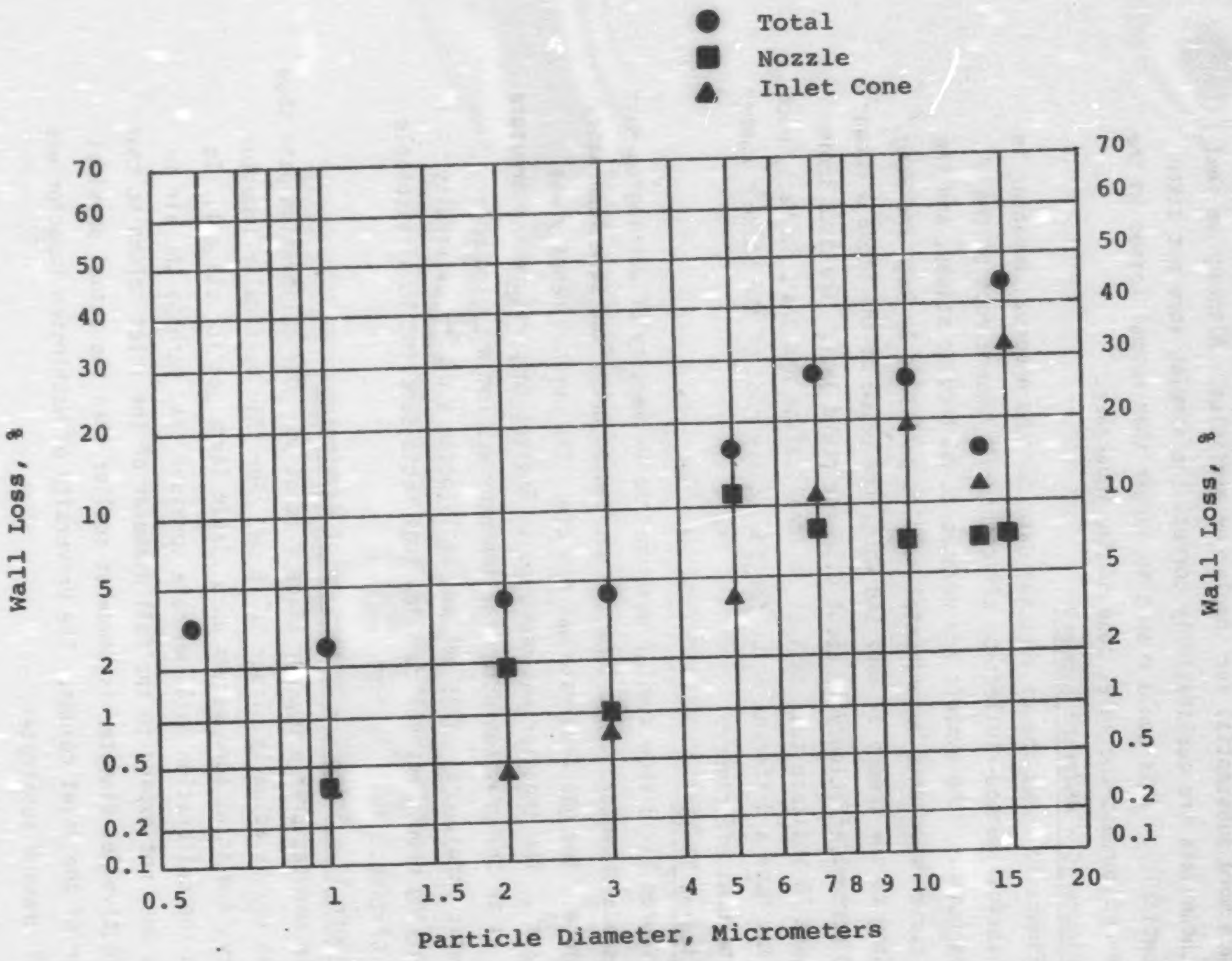


Figure 9. Andersen wall losses.

the impactor is found in the nozzle and inlet cone. Also, the losses are much more substantial for the larger particles. Although we feel that these data are qualitatively correct the samples were not taken isokinetically. This could result in larger than normal losses in the nozzle and perhaps the inlet cone of the impactors.

Figure 10 - Andersen Summary

Figure 10 shows the calibration data for the Andersen impactor in the classical method--collection efficiency vs. square root of the Stoke's number. The symbols are related to particular stages, and the solid curve represents experimental results published by Ranz and Wong.[2] According to the theory of Ranz and Wong, the value of the Stoke's number for 50 percent efficiency is about 0.38 for round jets. The data shown in Figure 10 indicate that this is an idealization and that, in fact, each stage may have a different value for the square root of the Stoke's number at 50 percent efficiency.

Figures 11-18

Figures 11-18 show similar data for the University of Washington Mark III Cascade Impactor. This impactor was tested under the same laboratory conditions. The gas flow rate was 0.5 cfm. The aerosol density was 1.35 g/cm^3. The impactor was operated at ambient temperature and pressure. Again, as in the previous data, the impactor was not operated under isokinetic conditions. This was primarily because of the difficulty in supplying enough aerosol to allow isokinetic sampling in a reasonable period of time.

Figure 11 - Stage 1 - University of Washington

The manufacturer's D_{50} for stage 1 is 34 μm. Our calibration data show that the D_{50} is actually closer to 15 μm. For this particular impactor geometry, the inlet cone serves as a single large jet for stage 1. To explain the calibration data, we have surmised that perhaps the air in the jet does not expand to the full diameter of the inlet column so that the effective jet diameter is somewhat smaller than the actual physical diameter of the inlet column. The University of Washington impactor was run with greased substrates.

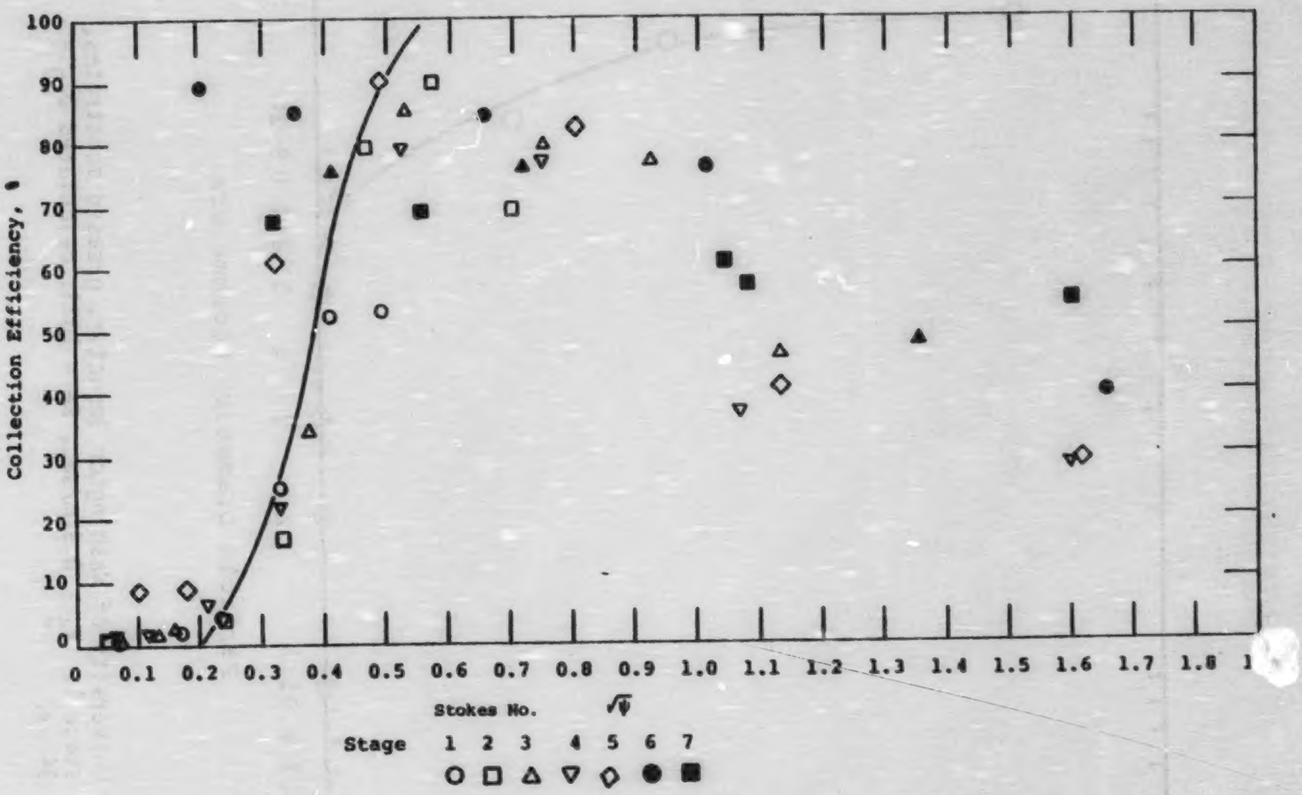


Figure 10. Summary--Andersen Impactor, glass fiber substrates.

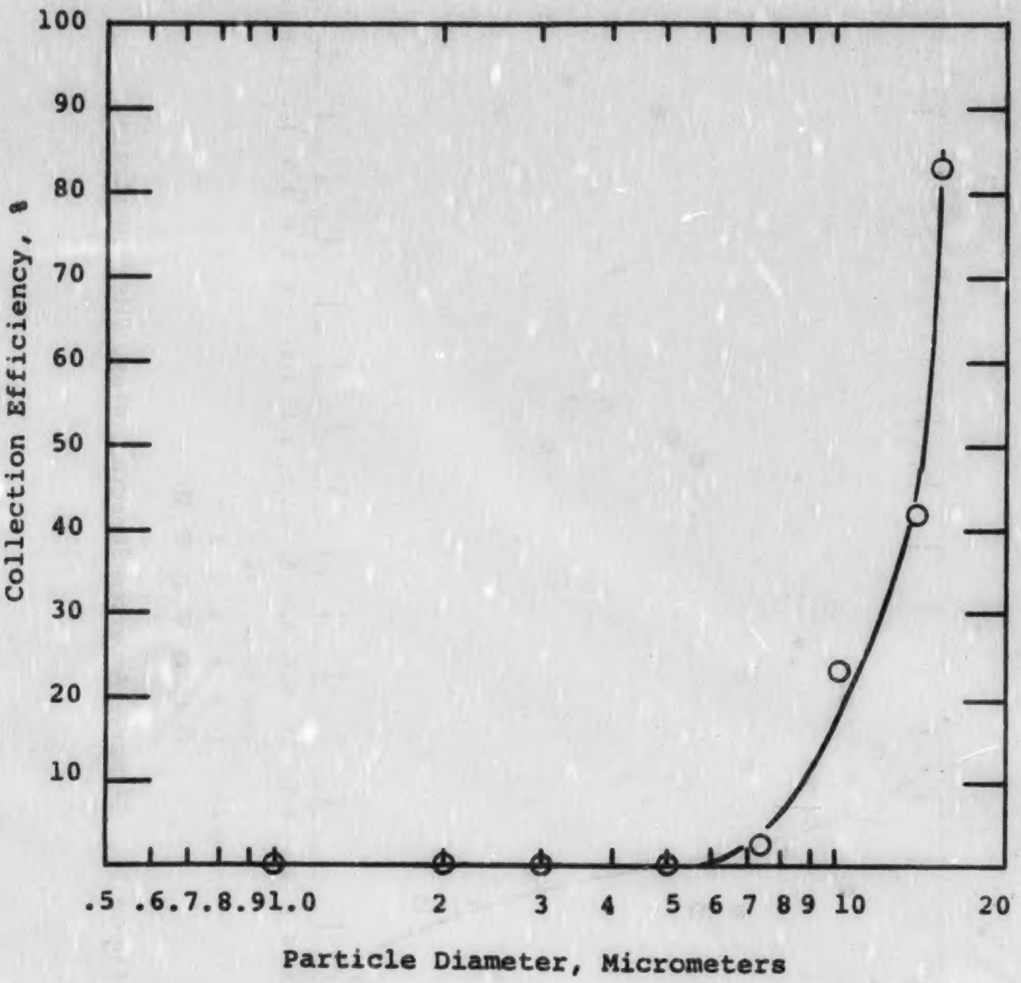


Figure 11. University of Washington Impactor, greased substrates, stage 1. V_j = 0.9 m/sec, Re = 1073, calculated D_{50} = 34 µm.

Figure 12 - Stage 2 - University of Washington

Figure 12 shows calibration data for stage 2 of the University of Washington impactor. In this case the manufacturer's D_{50} is 13 μm, while the calibration data show the D_{50} to be 10 μm.

Figure 13 - Stage 3 - University of Washington

In this case the manufacturer's D_{50} is 5.6 μm. There is some scatter in our calibration data, but nevertheless the indication is that the measured D_{50} is very near the manufacturer's calculated D_{50}.

Figure 14 - Stage 4 - University of Washington

Stage 4 is remarkably well-behaved. The efficiency rises to well above 90 percent and remains at a high value for a large range of particle sizes. Again, however, the calibration data indicate that the D_{50} is significantly smaller than the 2.7 μm value given by the manufacturer.

Figure 15 - Stage 5 - University of Washington

Stage 5 is a well behaved stage. The stage collection efficiency rises to a value near 90 percent and exhibits a very wide peak before the efficiency turns down for large particles. The velocity of 12.6 m/sec is approaching a value where the data indicate that scouring and bounce can occur. Although we do not feel that our data are especially accurate below 1 μm, the indications are that the actual D_{50} is somewhat lower than the manufacturer's published value of 1.4 μm.

Figure 16 - Stage 6 - University of Washington

The calibration data for stage 6 are incomplete because of our inability to calibrate stages below 1 μm. Apparently the peak efficiency is well above 90 percent, even though the jet velocity is 24.2 m/sec. This may indicate that the use of grease as an impaction substrate allows operation at higher jet velocities. A second peak is observed for large particles. The reason for this second peak is not understood at this time.

Figure 17 - Wall Losses for the University of Washington Impactor

Wall losses measured for the University of Washington impactor are similar to those measured for the other impactors. The losses are much more significant for large particles, and the bulk of the material was found within the nozzle and the inlet column.

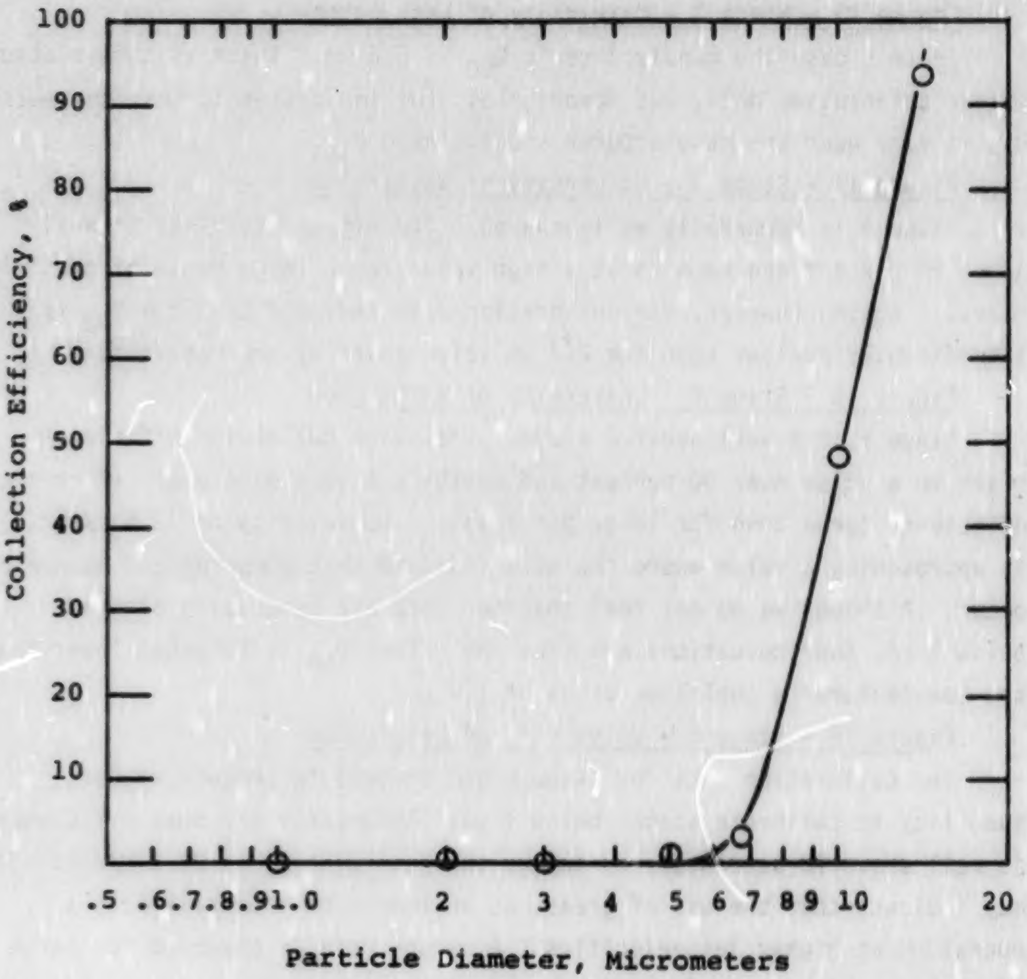


Figure 12. University of Washington Impactor, greased substrates, stage 2. V_j = 1.5 m/sec, Re = 562, calculated D_{50} = 13 μm.

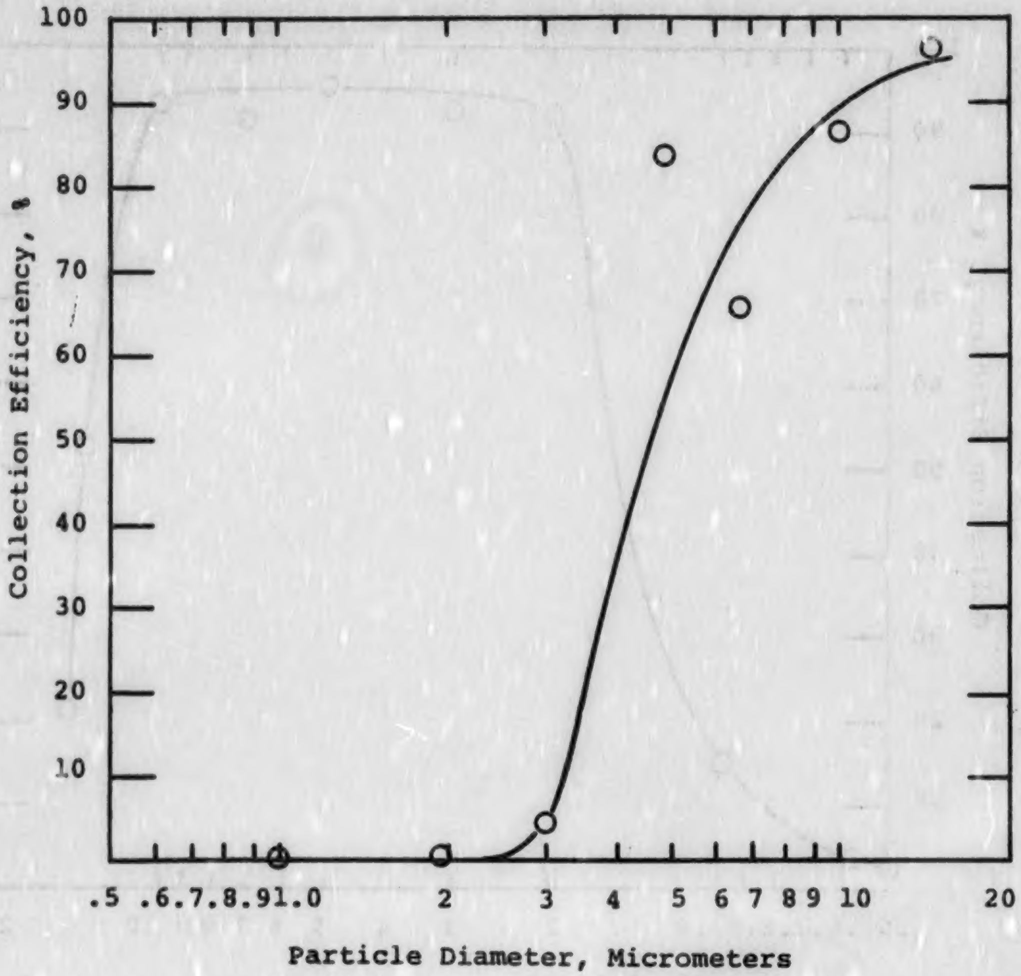


Figure 13. University of Washington Impactor, greased substrates, stage 3. V_j = 4.1 m/sec, Re = 650, calculated D_{50} = 5.6 μm.

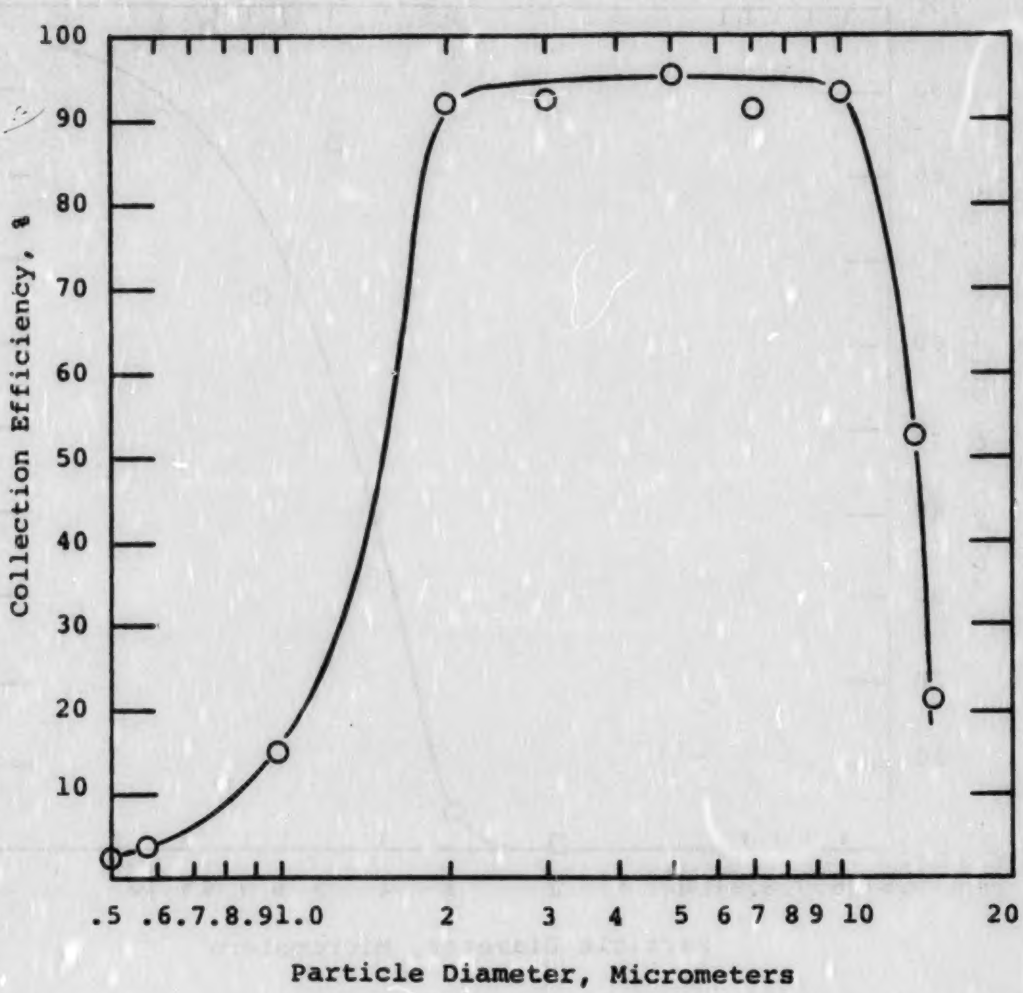


Figure 14. University of Washington Impactor, greased substrates, stage 4. V_j = 5.0 m/sec, Re = 264, calculated D_{50} = 2.7 μm.

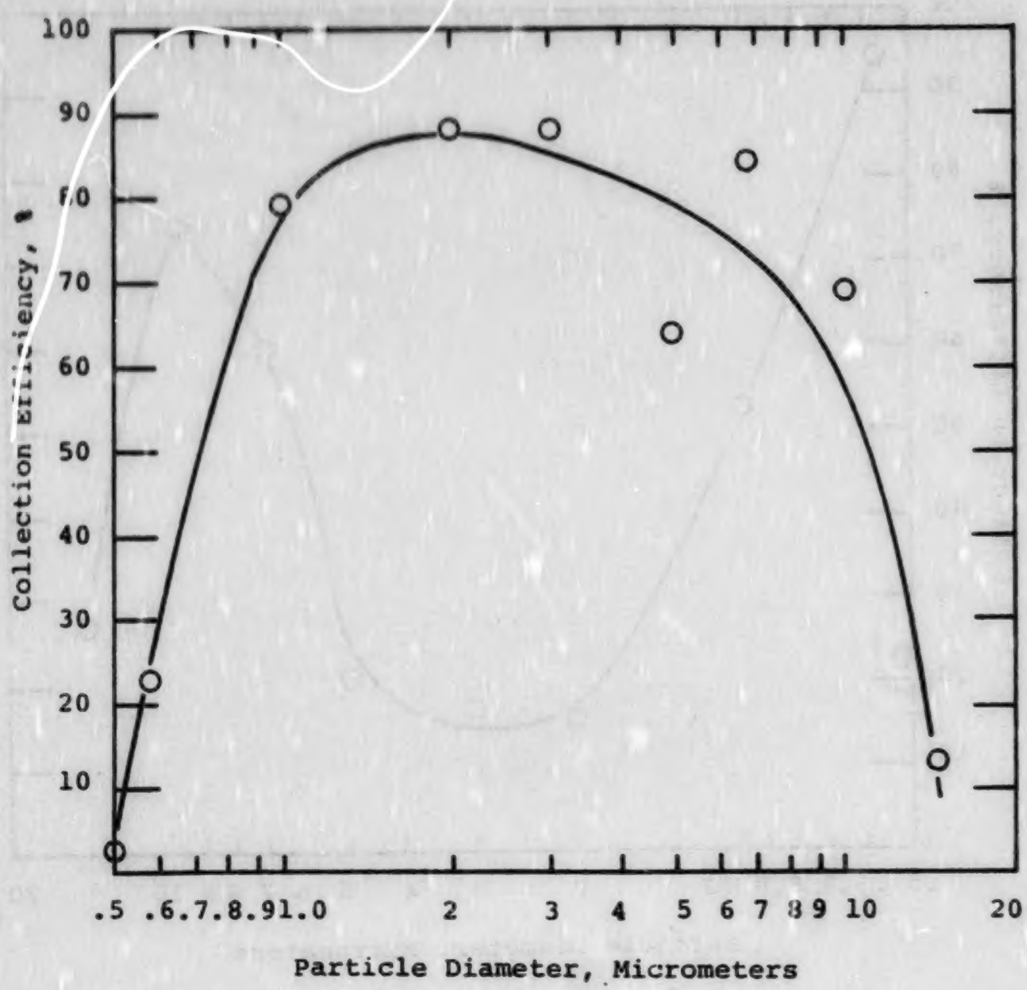


Figure 15. University of Washington Impactor, greased substrates, stage 5. $V_j = 12.6$ m/sec, Re = 377, calculated $D_{50} = 1.4$ μm.

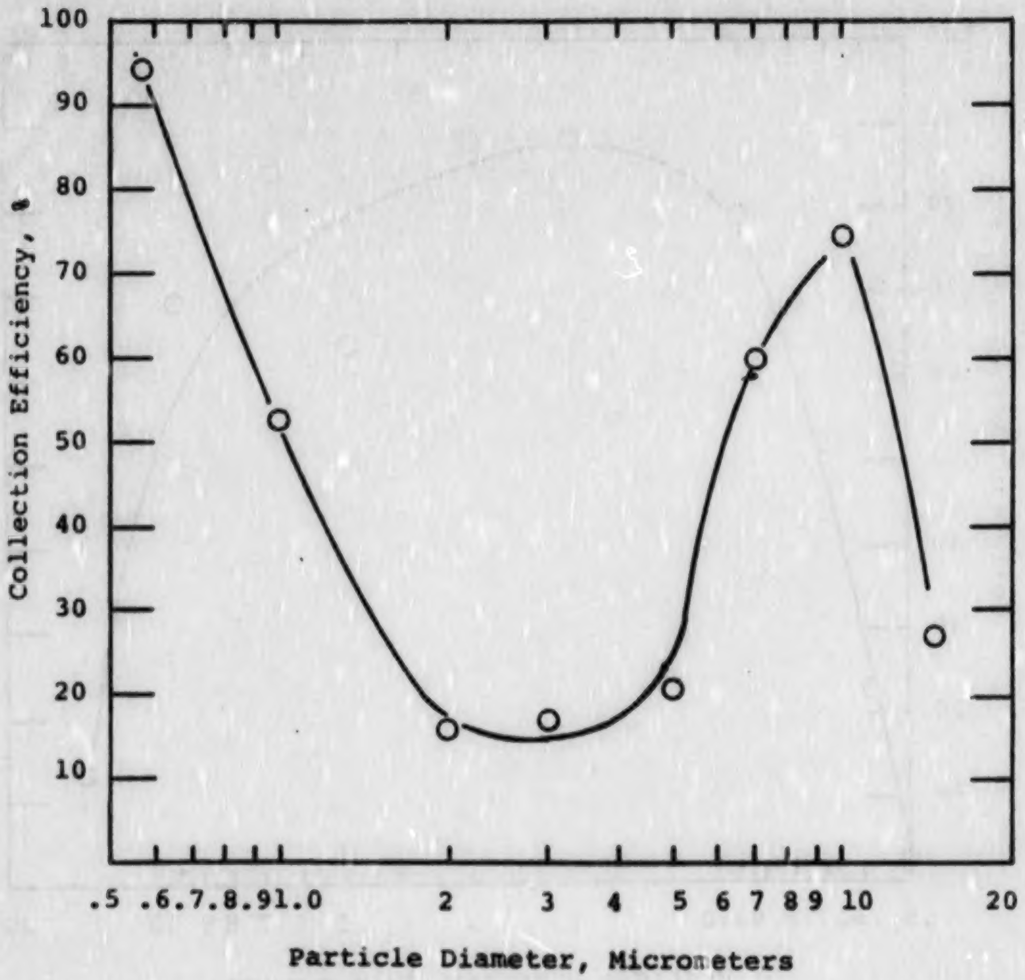


Figure 16. University of Washington Impactor, greased substrates, stage 6. V_j = 24.2 m/sec, Re = 522, calculated D_{50} = 0.69 μm.

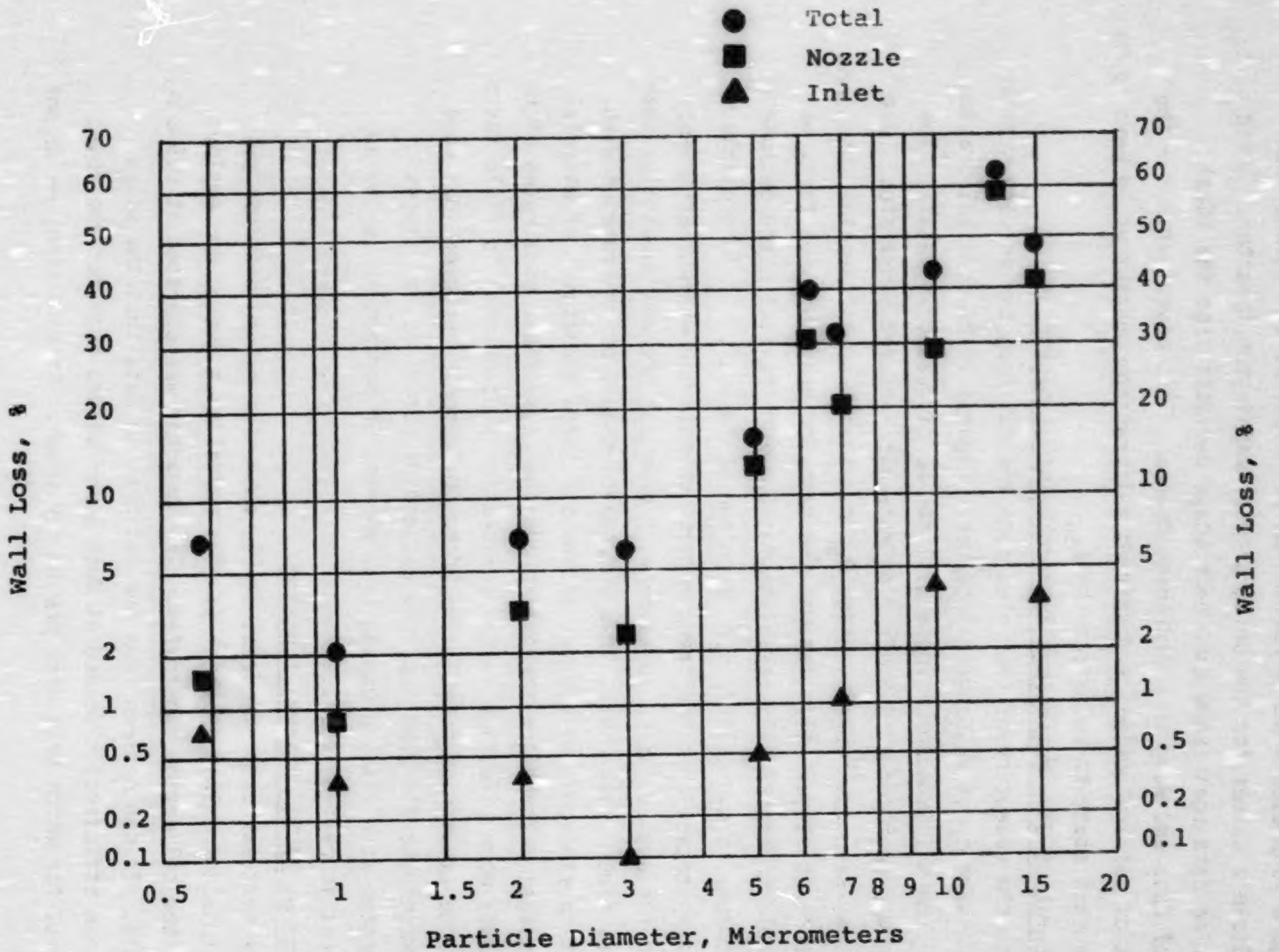


Figure 17. University of Washington wall losses.

Figure 18 - Summary - University of Washington Impactor

Figure 18 presents stage collection efficiencies vs. square root of the Stoke's number for the University of Washington impactor. These calibration data again show that each stage deviates from the ideal theory of Ranz and Wong to a different degree. This means , that for the purposes of data reduction , a different calibration constant would have to be used for each stage to calculate the D_{50}.

Figures 19-26 - Meterology Research, Inc. Impactor

With the exception of the inlet cone the MRI impactor is very similar to the University of Washington impactor. Figures 19-26 show calibration data for the MRI impactor. These will not be discussed in detail. The impactor was operated at 0.5 cfm flow rate with greased substrates. Jet velocities, Reynolds numbers, stage D_{50}'s, as well as calibration data, are given for each stage. Also, as was the case for previous impactors, we were unable to empirically establish the D_{50} for the last stage because of an inaccuracy in our calibration technique below 1 µm. It is possible, however, to notice that the trend repeats wherein the highest efficiency attained for a given stage is somewhat higher for greased substrates than for glass fiber substrates. This phenomenon should be interpreted with the fact in mind that we did not accumulate large quantities of material in the stages. Therefore the grease and the glass fiber substrates might be slightly more effective in our calibration studies than in field tests where large amounts of material would be collected. Lundgren[3] has done tests wherein he did find that the collection efficiency of stages deteriorated with time, dropping from perhaps 90 percent to as low as 70 percent in half an hour.

Figures 27-34 - Sierra Impactor

The next series of figures, 27-34, show calibration data for the Sierra impactor. This impactor is of a radial slit design and employs glass fiber collection substrates. The impactor was operated at 0.5 cfm flow rate. It can be seen from the calibration data that the stage collection efficiencies peaked at much lower values than for the round impactors, for which data were previously shown. At this time, we do not

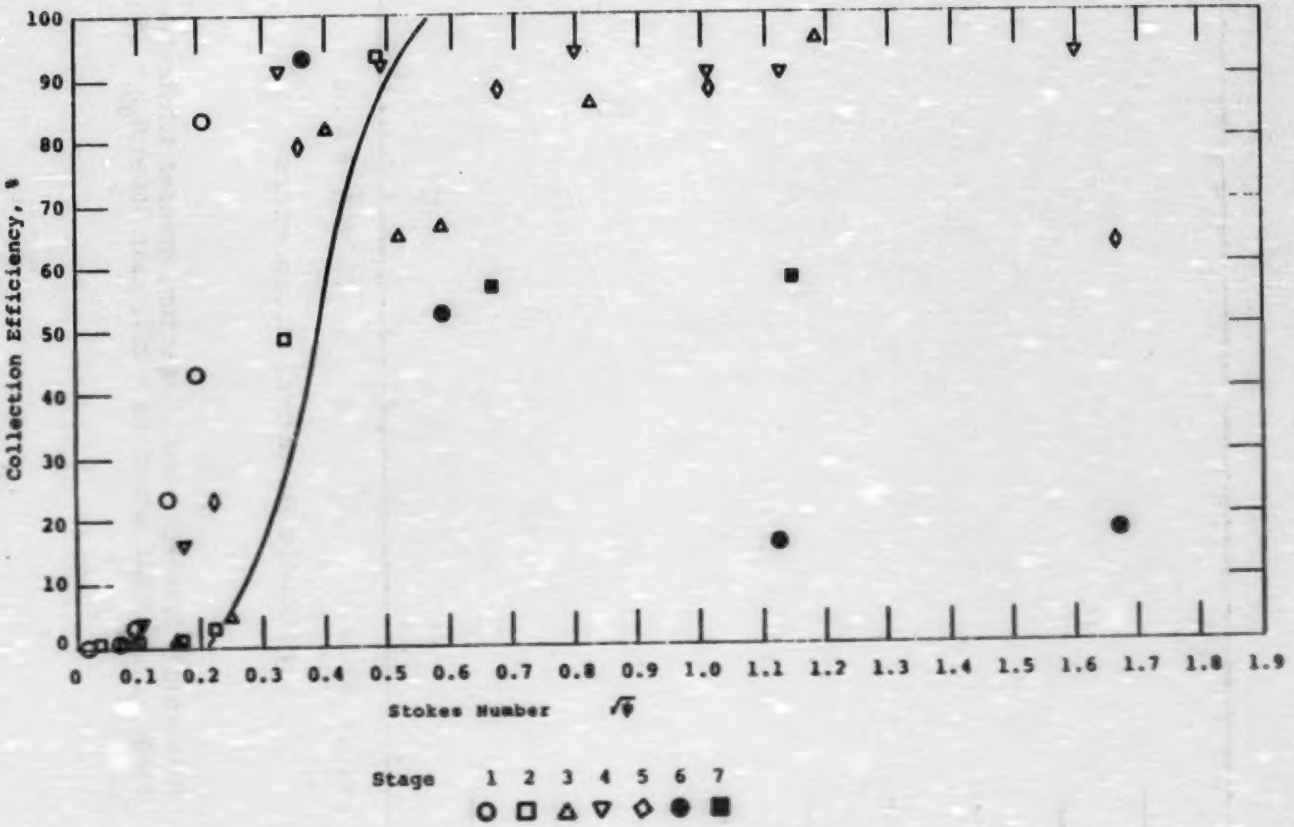


Figure 18. University of Washington Impactor, greased substrates, summary of calibration data.

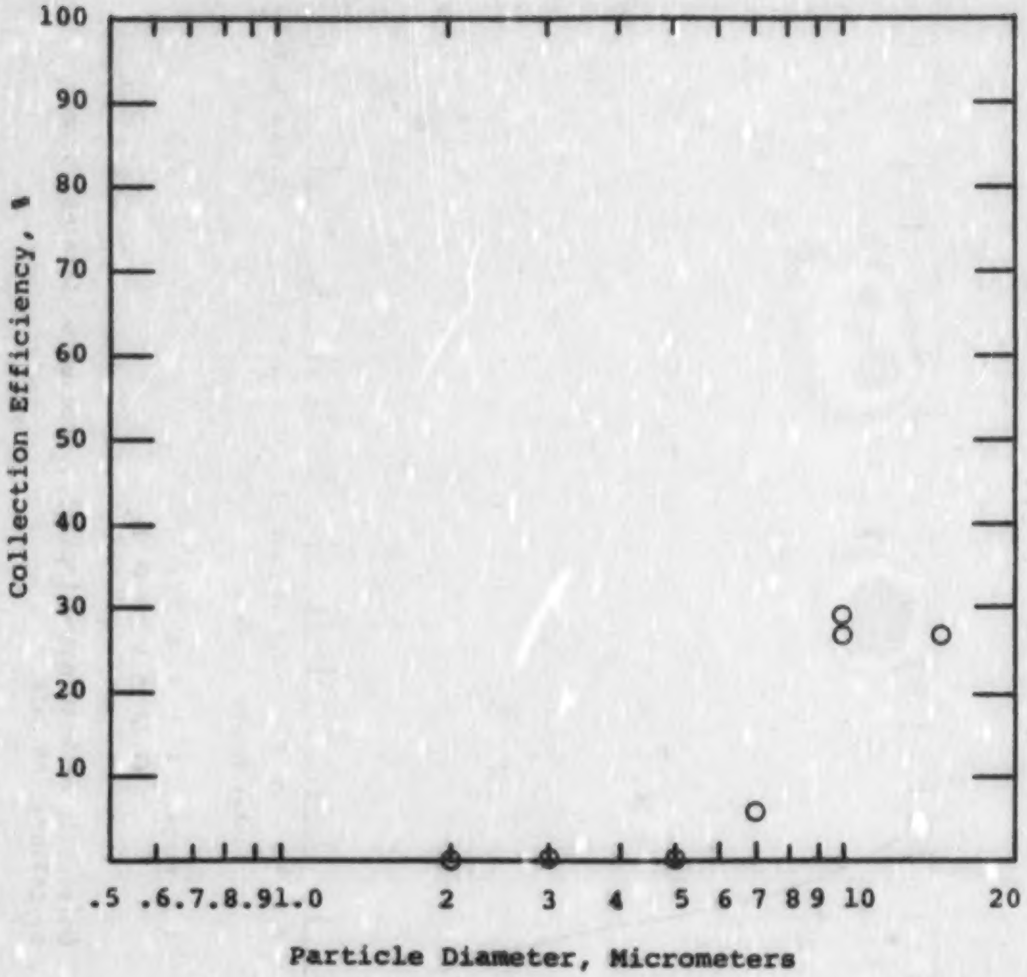


Figure 19. Meteorology Research, Inc., Impactor, greased substrates, stage 1. V_j = 0.5 m/sec, Re = 281, calculated D_{50} = 24 μm.

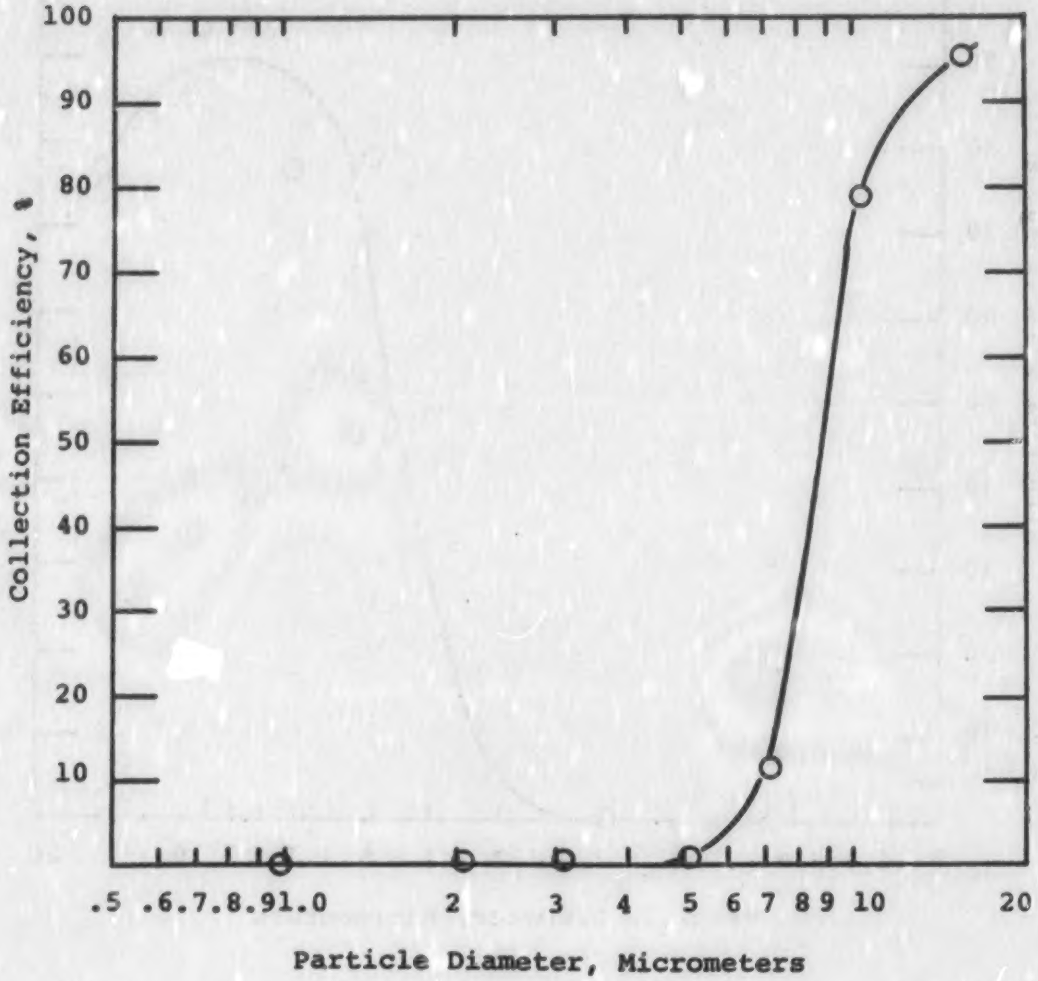


Figure 20. Meteorology Research, Inc., Impactor, greased substrates, stage 2. V_j = 1.1 m/sec, Re = 341, calculated D_{50} = 12 μm.

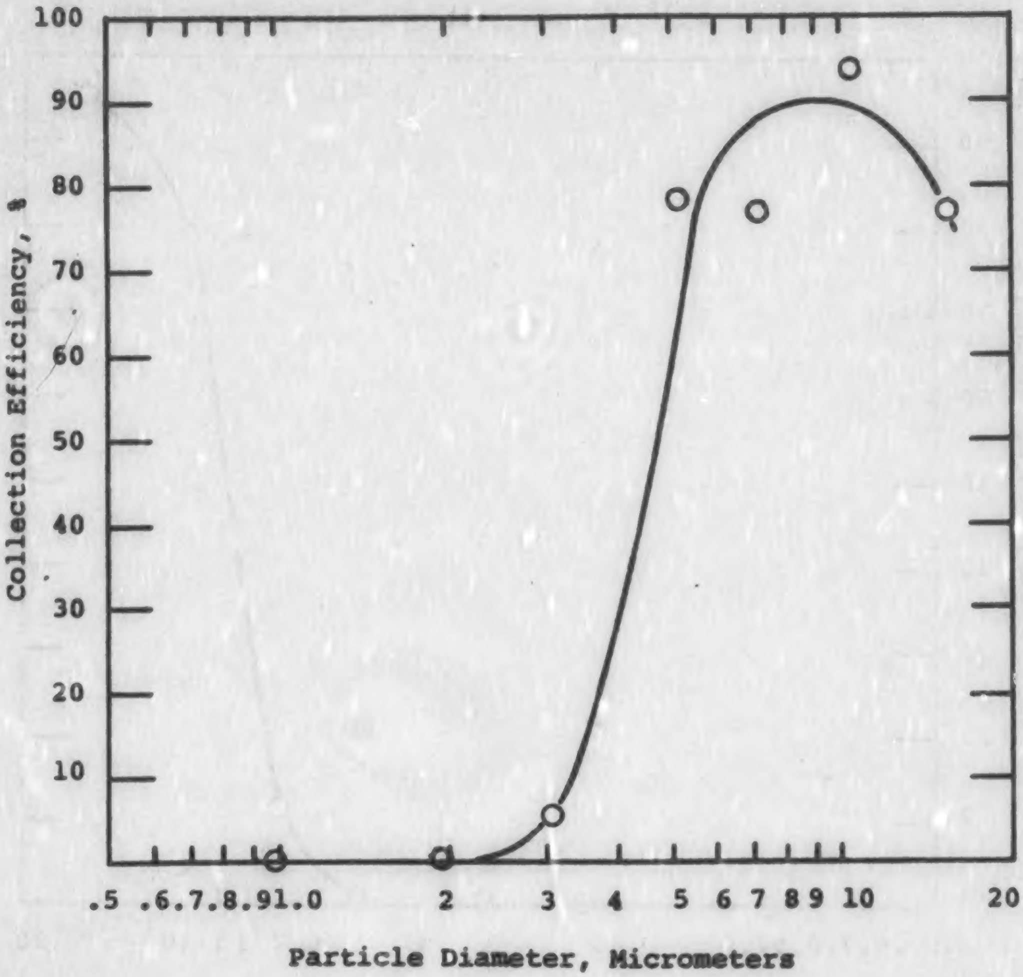


Figure 21. Meteorology Research, Inc., Impactor, greased substrates, stage 3. V_j = 3.2 m/sec, Re = 411, calculated D_{50} = 4.5 μm.

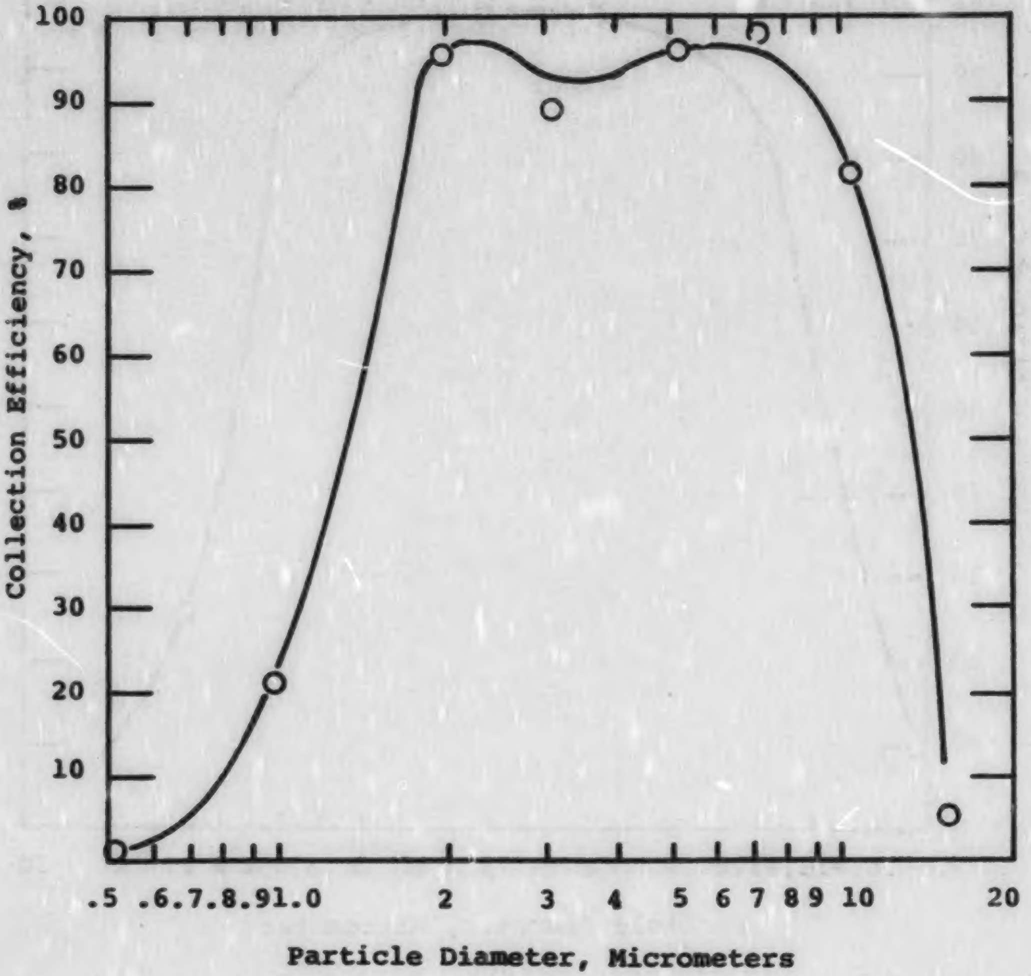


Figure 22. Meteorology Research, Inc., Impactor, greased substrates, stage 4. V_j = 9.0 m/sec, Re = 684, calculated D_{50} = 2.1 μm.

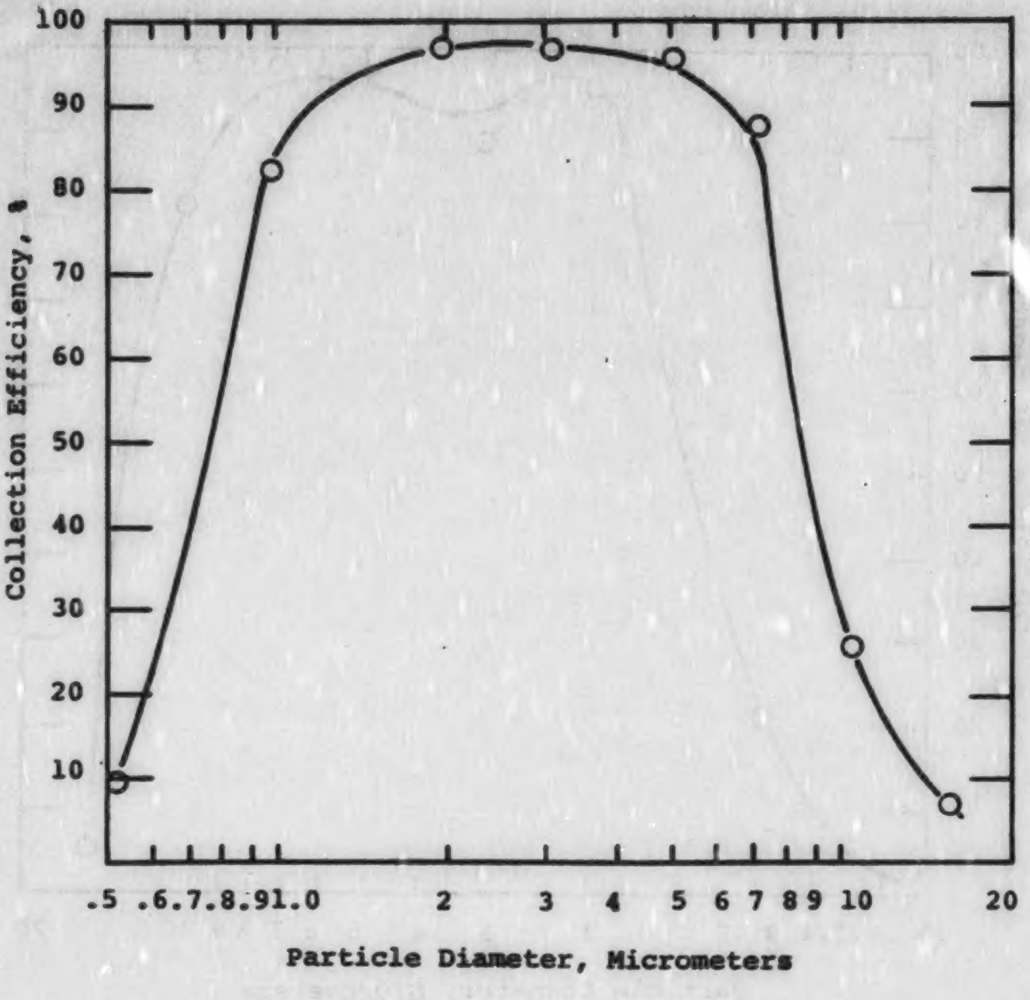


Figure 23. Meteorology Research, Inc., Impactor, greased substrates, stage 5. V_j = 18.2 m/sec, Re = 973, calculated D_{50} = 1.2 μm.

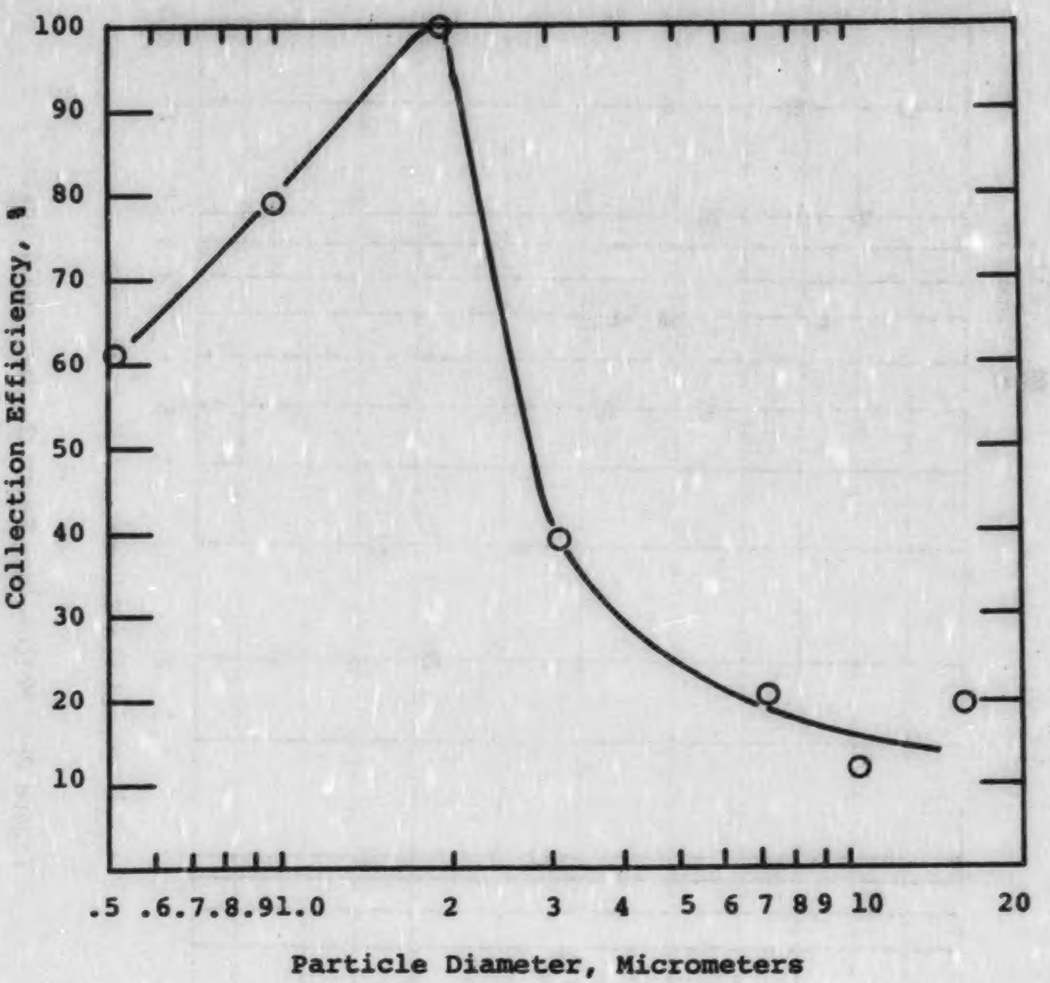


Figure 24. Meteorology Research, Inc., Impactor, greased substrates, stage 6. V_j = 46.0 m/sec, Re = 1529, calculated D_{50} = 0.57 μm.

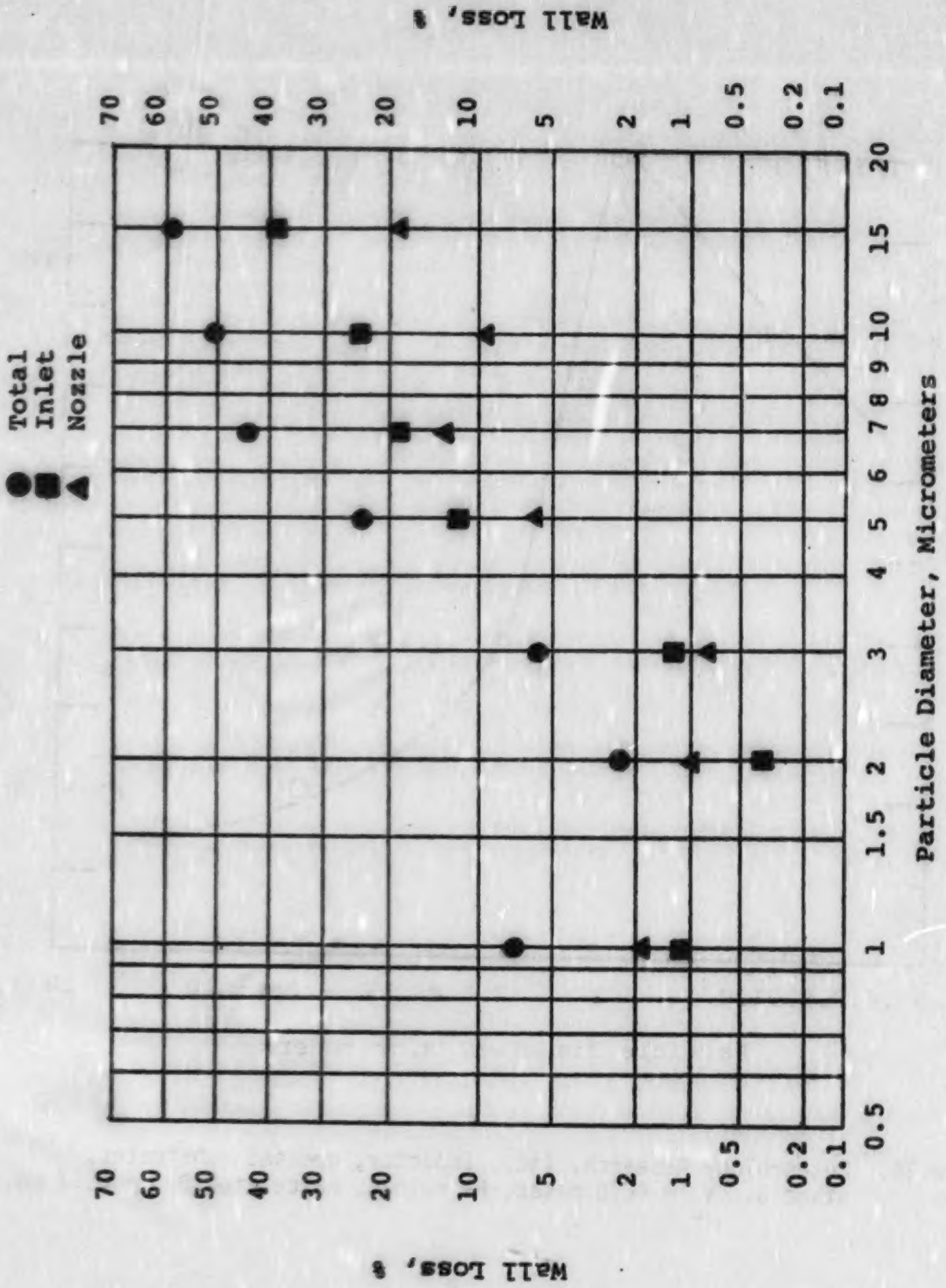


Figure 25. Meteorology Research, Inc., wall losses.

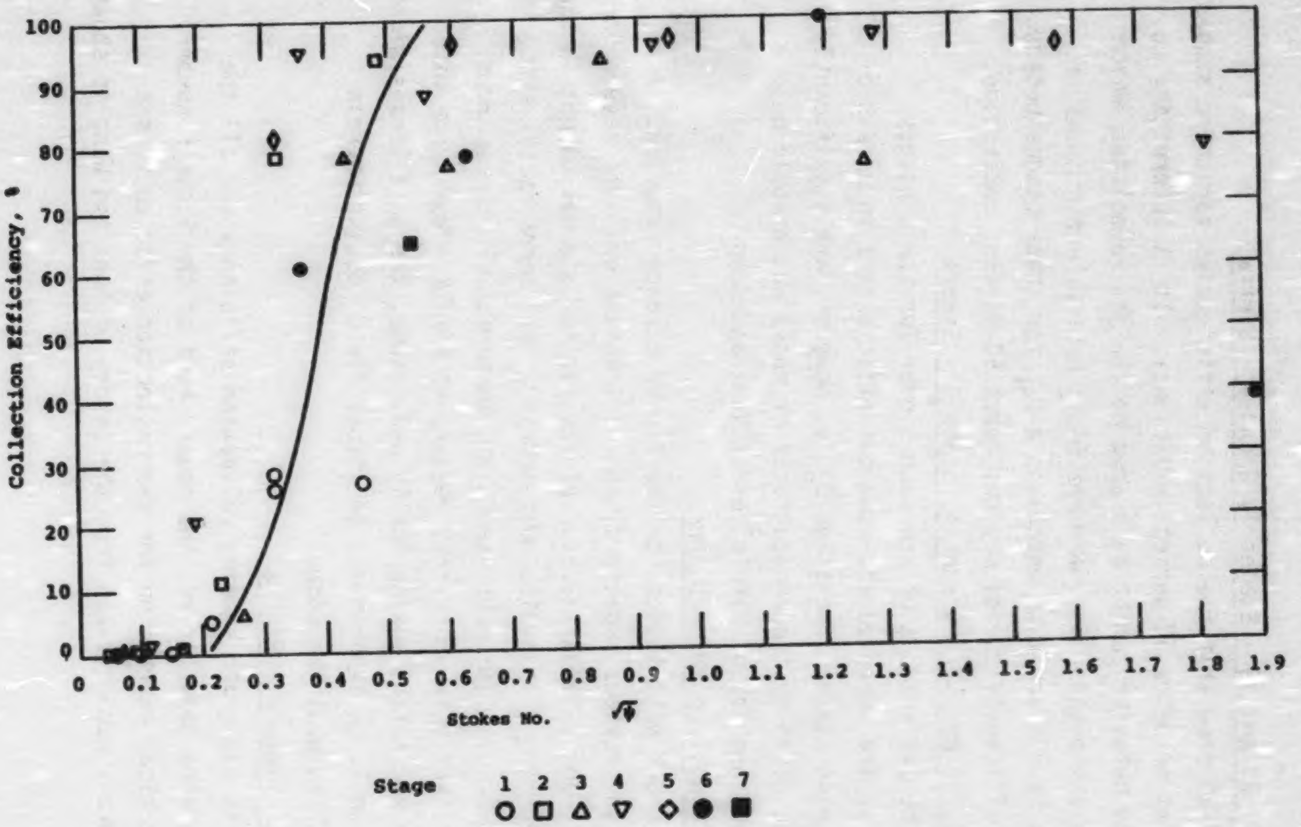


Figure 26. Summary--Meteorology Research, Inc., Impactor, greased substrates.

know if this is due to our operating the impactor at a nonoptimum flow rate, or if the nonideal behavior is a function of the radial slit design.

Figures 27, 28 and 29 - Stages 1, 2 and 3 - Sierra

Figures 27-29 show that the collection efficiencies for these stages peak somewhere at or below 50 percent efficiency. It is interesting to notice that this behavior cannot be blamed on the jet velocities, which are well within the bounds, that we have found tolerable for round jet geometries. It is difficult to establish a D_{50} for these stages because the collection efficiency curves may not reach 50 percent collection.

Figures 30, 31 and 32 - Stages 4, 5 and 6 - Sierra

Although the jet velocities are much higher for these stages efficiency curves are improved with maximum efficiencies in excess of 80 percent for each stage. The stage D_{50}'s, however, are significantly lower than those given by the manufacturer or those which would be predicted by the theories of Marple[4] or of Ranz and Wong.

Figure 33 - Wall Losses - Sierra

Figure 33 shows wall losses for the Sierra cascade impactor. As was the case with previous impactors, the wall losses are more severe for large particles, and a large fraction of the particulate was caught in the nozzle and inlet cone. John Olin, who designed the Sierra radial slit impactor, has shown me some data wherein he measured wall losses which were much lower than this. For that reason, we should reemphasize that these data were not taken isokinetically and, hence, the wall losses might be larger than would be experienced in actual field testing wherein isokinetic sampling would be done.

Figure 34 - Summary - Sierra

Figure 34 is the graph showing collection efficiency for all the stages of the Sierra impactor vs. the square root of the Stoke's number. Also, the theoretical expression for the collection efficiency as a function of Stoke's number taken from the theory of Ranz and Wong is shown.

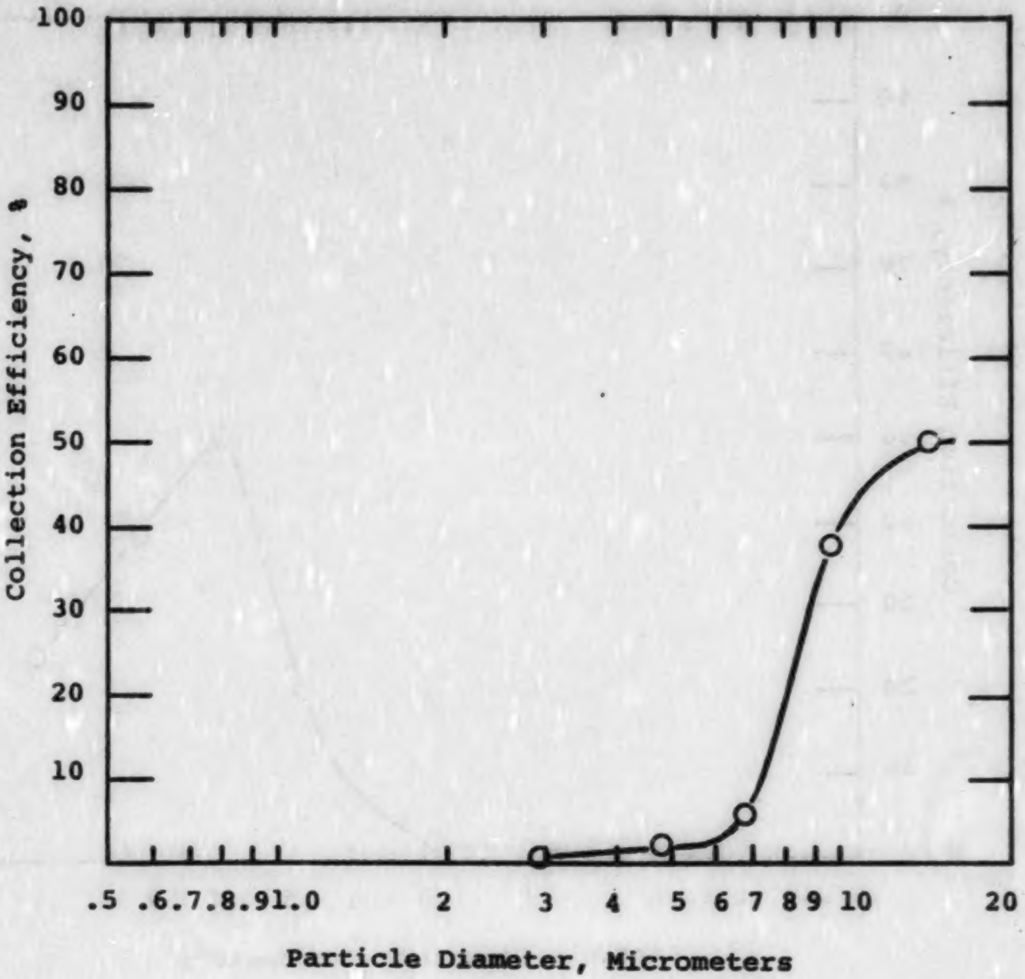


Figure 27. Sierra Cascade Impactor, glass fiber substrates, stage 1. V_j = 1.3 m/sec, Re = 555, calculated D_{50} = 15 μm.

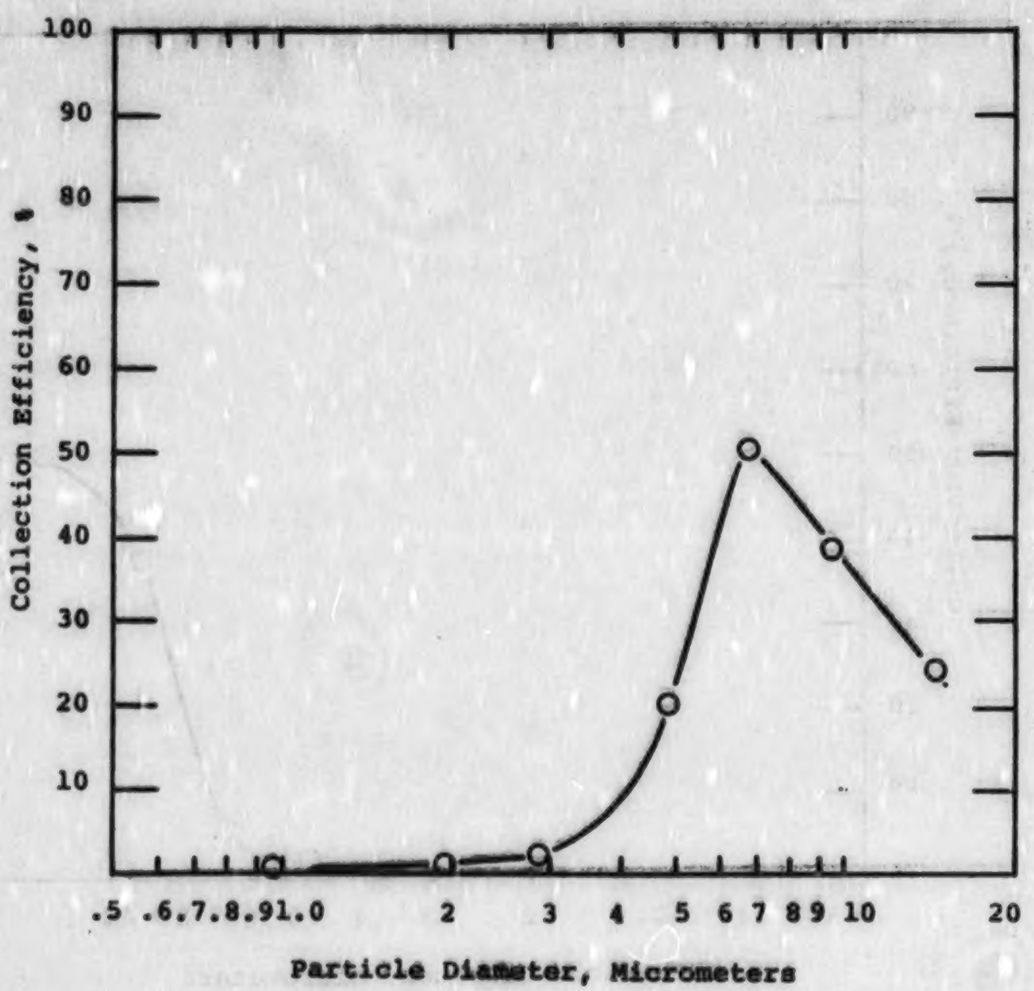


Figure 28. Sierra Cascade Impactor, glass fiber substrates, stage 2. V_j = 2.3 m/sec, Re = 571, calculated D_{50} = 8.2 μm.

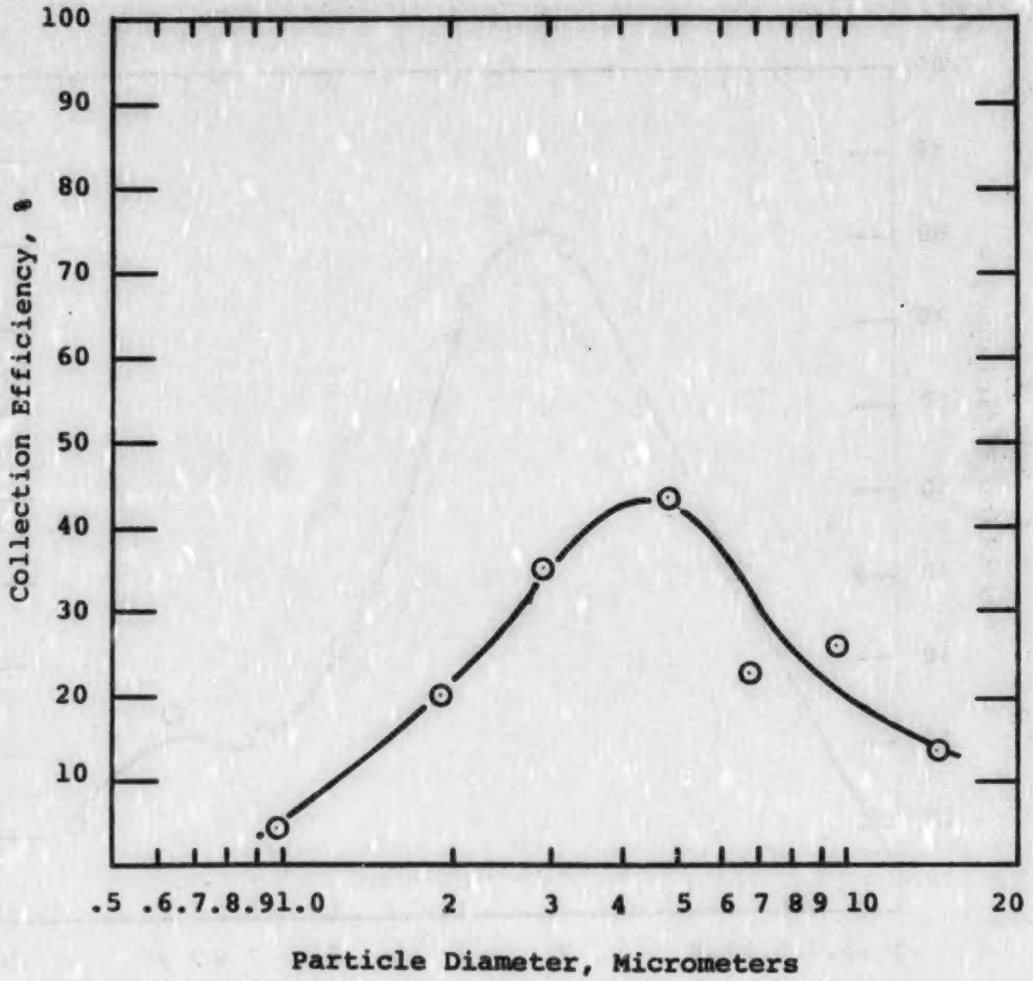


Figure 29. Sierra Cascade Impactor, glass fiber substrates, stage 3. V_j = 5.4 m/sec, Re = 763, calculated D_{50} = 3.7 µm.

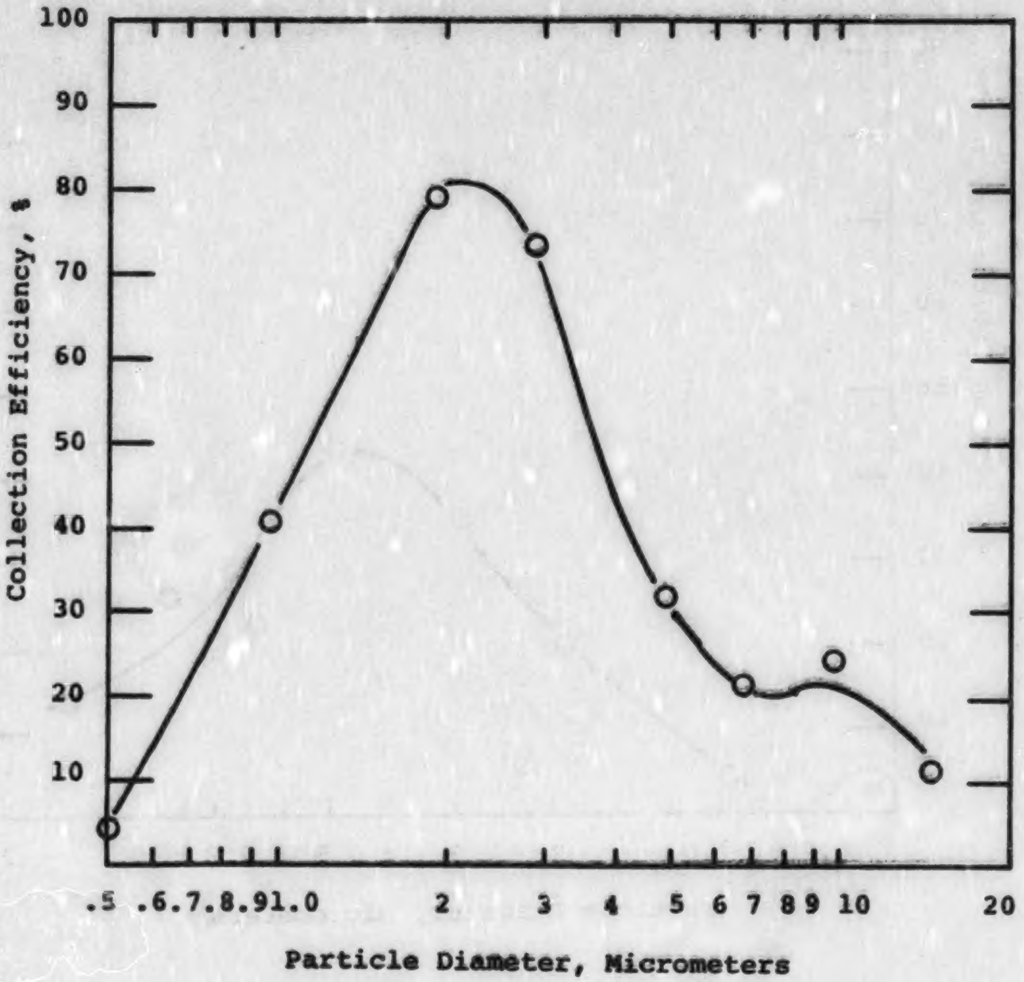


Figure 30. Sierra Cascade Impactor, glass fiber substrates, stage 4. V_j = 9.6 m/sec, Re = 773, calculated D_{50} = 2.3 μm.

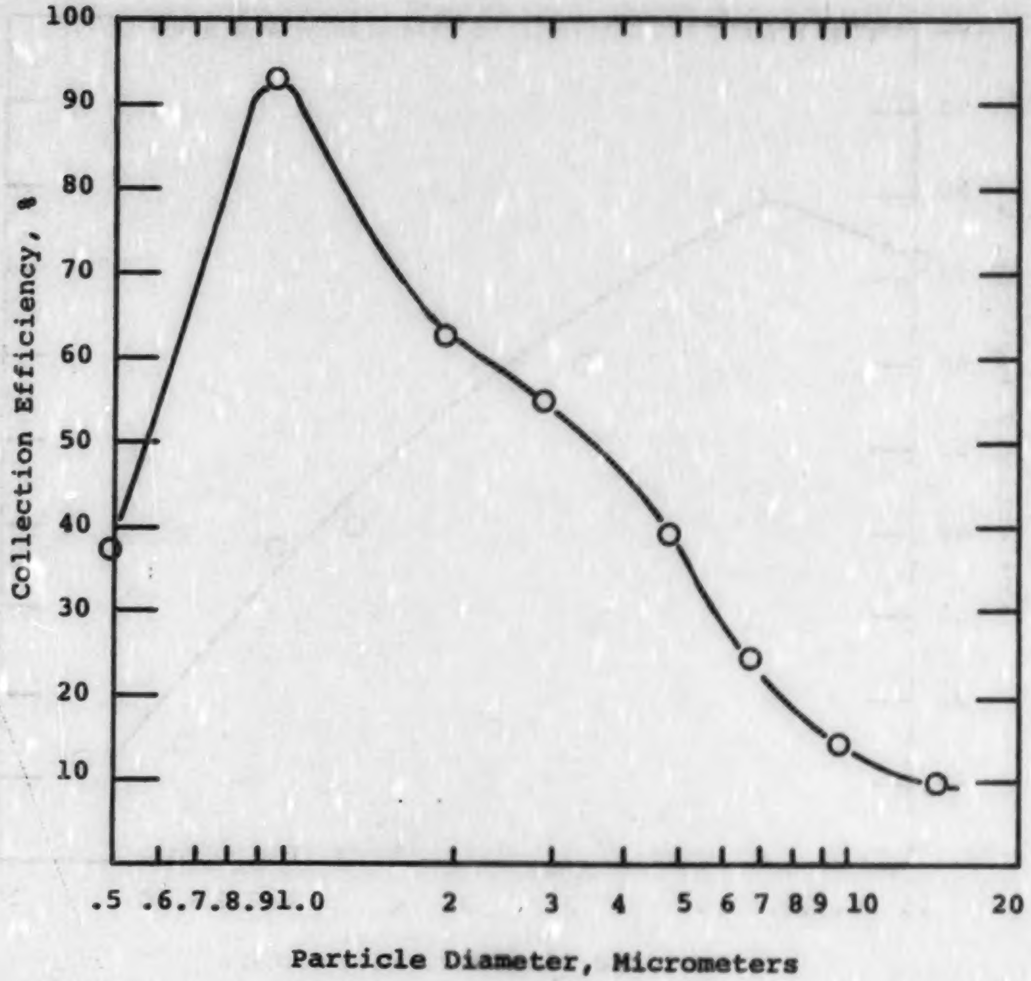


Figure 31. Sierra Cascade Impactor, glass fiber substrates, stage 5. V_j = 17.1 m/sec, Re = 778, calculated D_{50} = 1.1 μm.

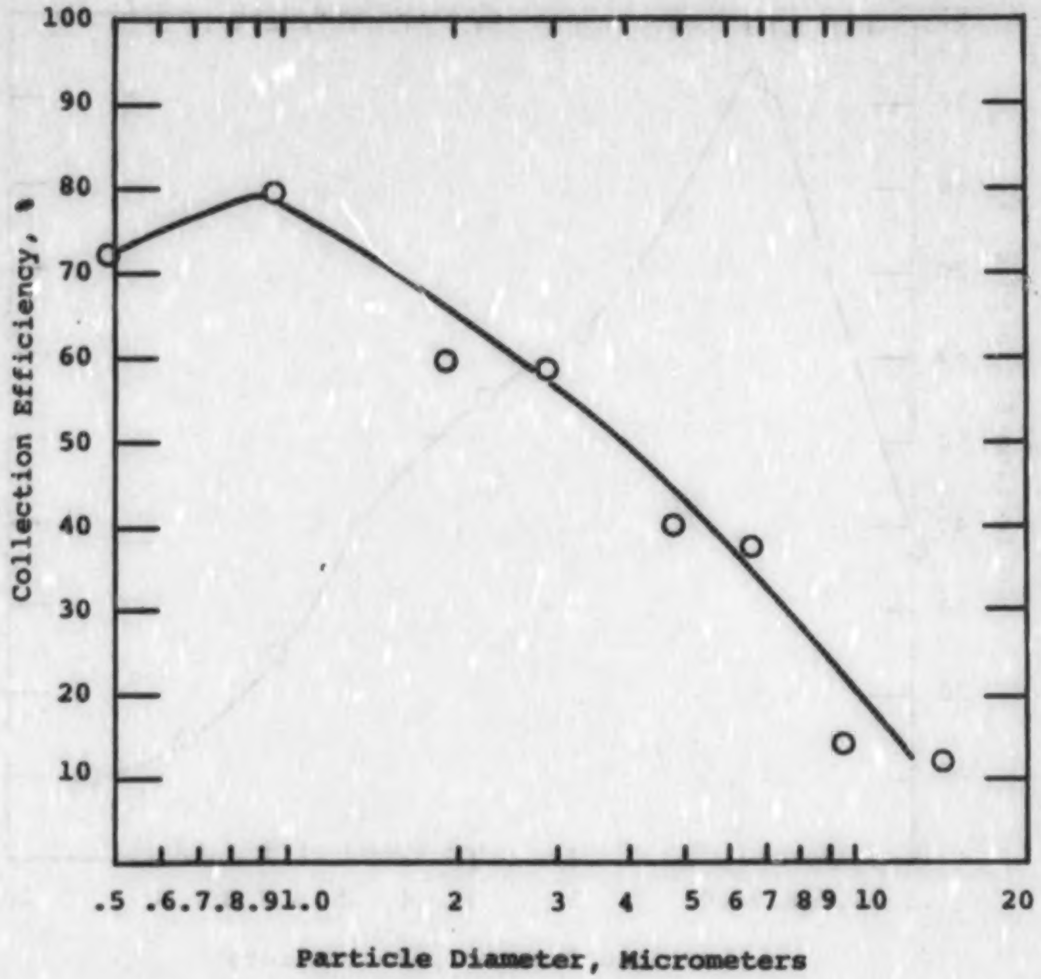


Figure 32. Sierra Cascade Impactor, glass fiber substrates, stage 6. V_j = 42.1 m/sec, Re = 1308, calculated D_{50} = 0.58 μm.

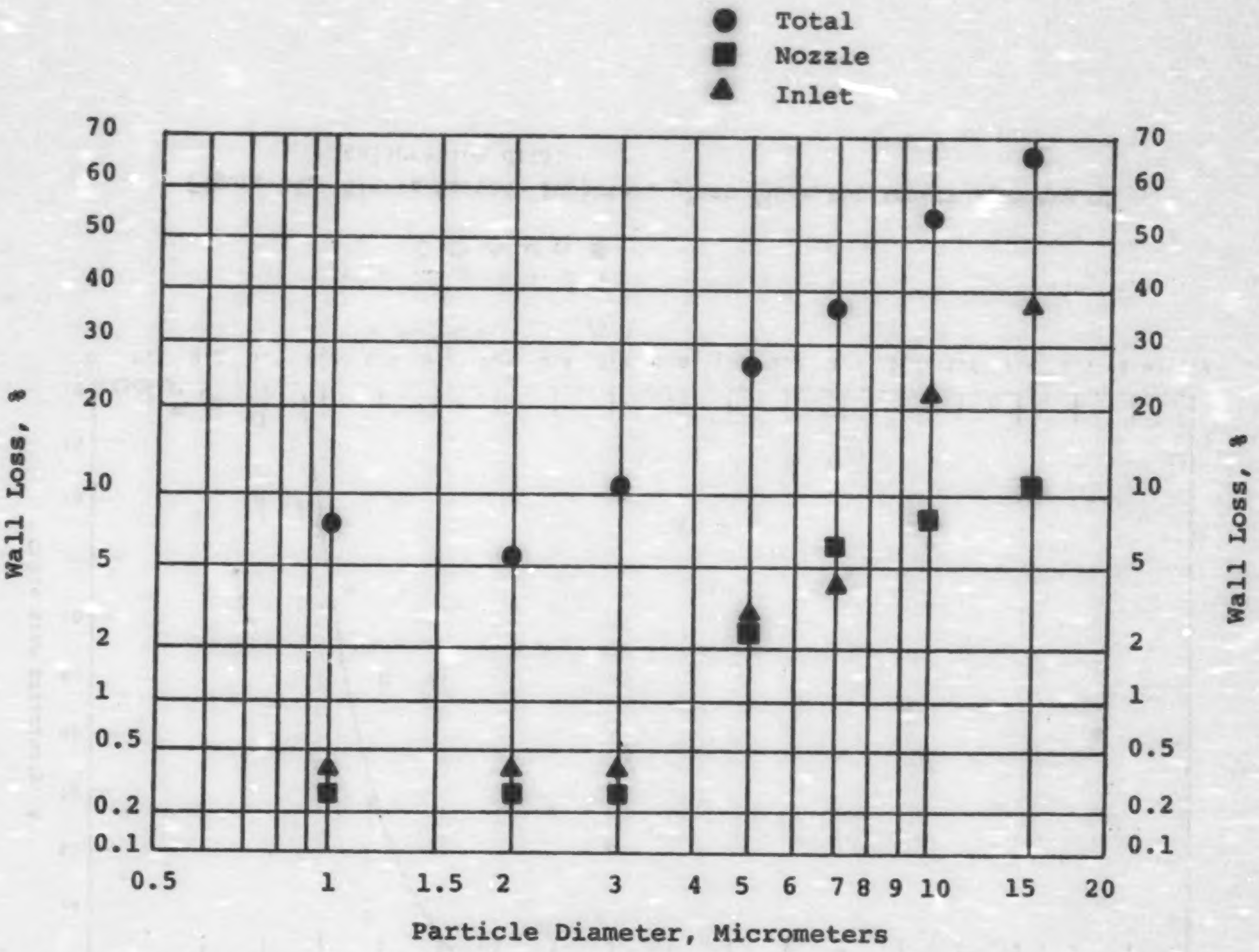


Figure 33. Sierra wall losses.

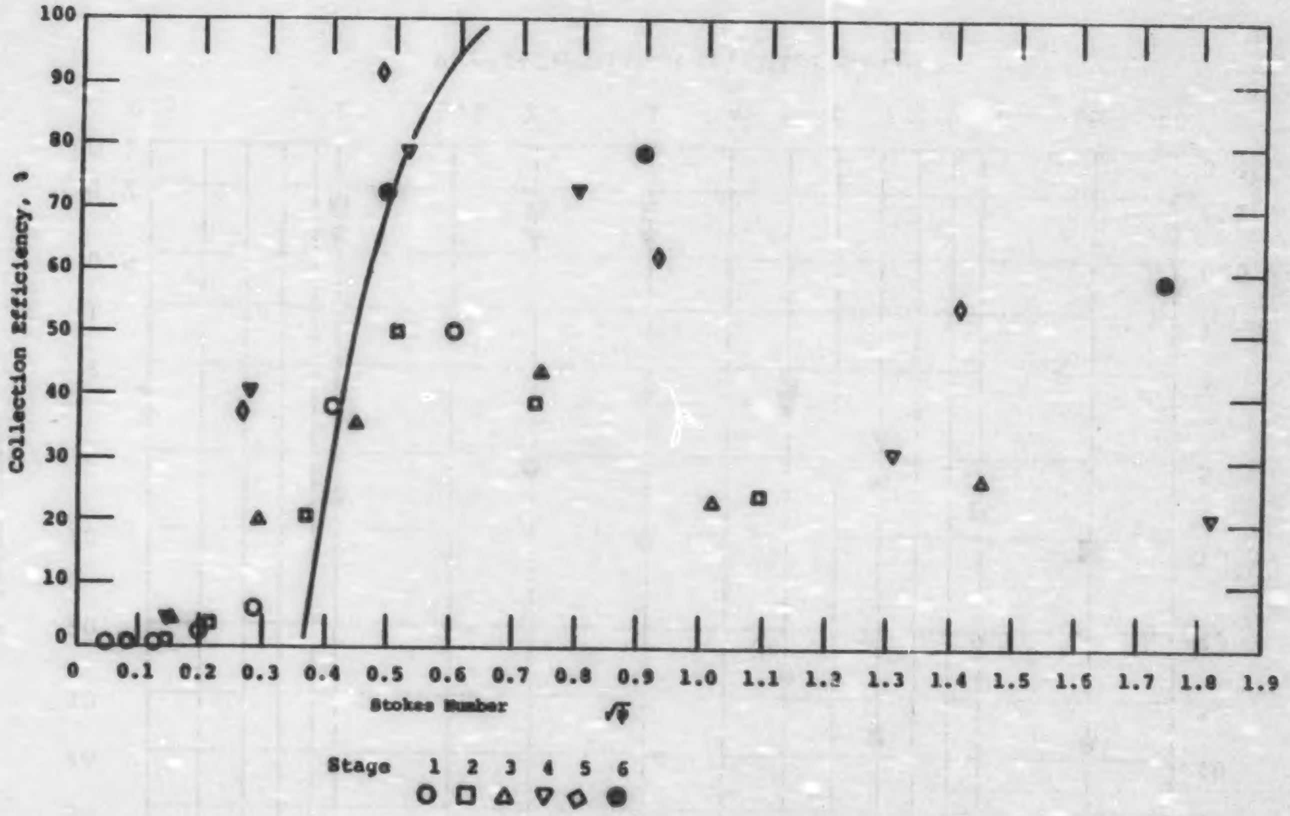


Figure 34. Sierra Cascade Impactor, glass fiber substrates, summary of calibration data.

Figures 35-43 - Brink Cascade Impactor

Figures 35-43 show data for the Brink cascade impactor. We have modified this impactor to include: (1) an in-line cyclone, (2) a zero stage which is included in the cyclone above the first stage furnished by the manufacturer, and (3) a sixth stage which is located downstream from the smallest stage furnished by the manufacturer. The design flow rate for this impactor is 0.12 cfm; our tests were done at about 0.03 cfm. With these modifications, the Brink impactor can be run at a low flow rate and gives longer sampling times for better averaging where dust or particulate loadings are heavy. Also, the Brink impactor is the only impactor for which we have experience with both grease and glass fiber substrates. Consequently, included in our calibration data are results taken using both types of substrates. Data taken with glass fiber substrates are shown as open circles while data taken with grease substrates (greased aluminum foils) are shown as closed circles.

Figure 35 - Stage 0 - Brink

Figure 35 shows data for the Brink impactor used with both glass fiber and greased foil substrates. As shown in the figure, the manufacturer's D_{50} is about 9.5 μm. Our calibration data with ammonium fluorescein however, show the D_{50} to be approximately 8.5 μm.

Figure 36 - Stage 1 - Brink

This figure shows calibration data for stage 1, which is the stage with the largest cut point furnished by the manufacturer. The manufacturer's D_{50} for our test conditions would be 5.4 μm, while our calibration data indicate the D_{50} would be slightly smaller than this. We do not have adequate resolution to give the D_{50} more precisely. Notice that the peak efficiency for the greased substrate is near 100 percent while the peak efficiency for the glass fiber substrate is slightly over 90 percent. In this case our data indicate that the D_{50} for both substrates was approximately the same.

Figure 37 - Stage 2 - Brink

Figure 37 shows our calibration data for stage 2 for the Brink impactor. In this case, once again the greased substrate shows a maximum collection efficiency well in excess of 90 percent, while the glass fiber

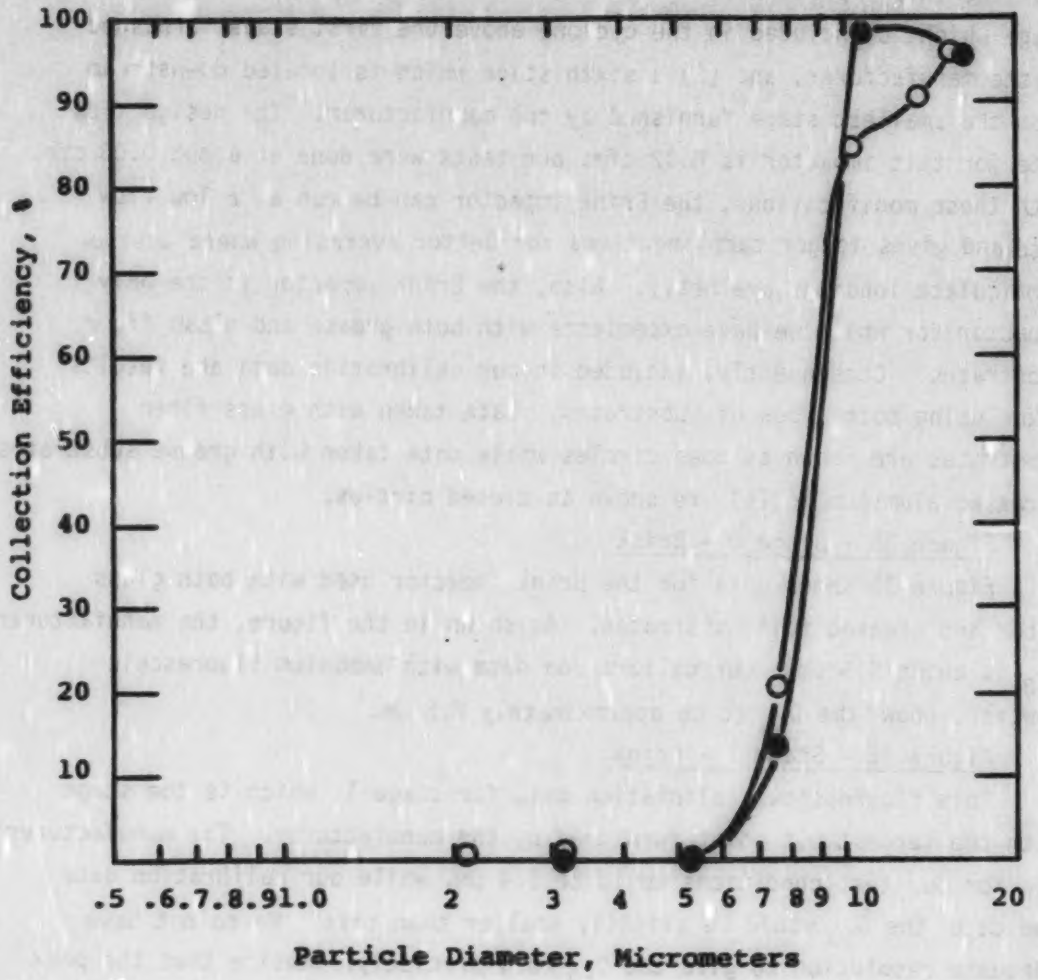


Figure 35. Modified Brink Impactor, glass fiber substrates, stage 0. V_j = 1.4 m/sec, Re = 470, calculated D_{50} = 9.5 µm.

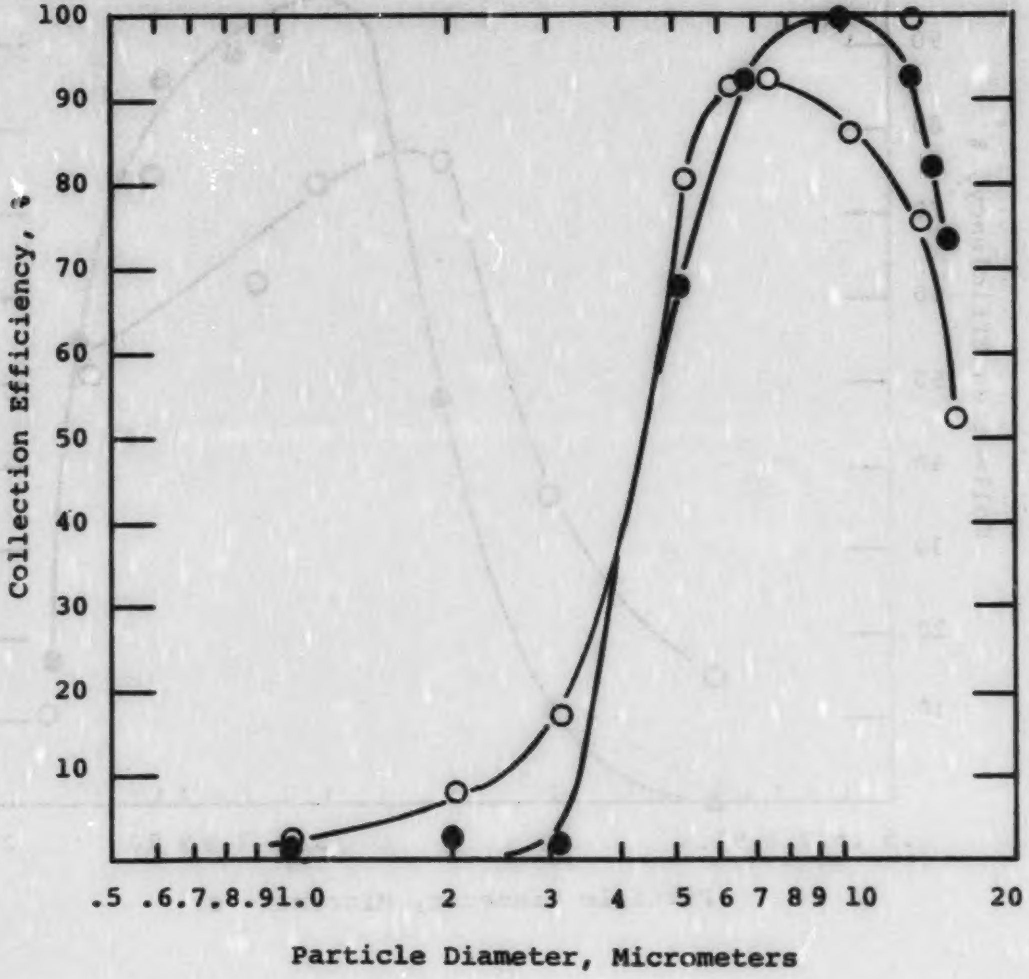


Figure 36. Modified Brink Impactor, glass fiber substrates, stage 1. V_j = 2.9 m/sec, Re = 470, calculated D_{50} = 5.4 μm.

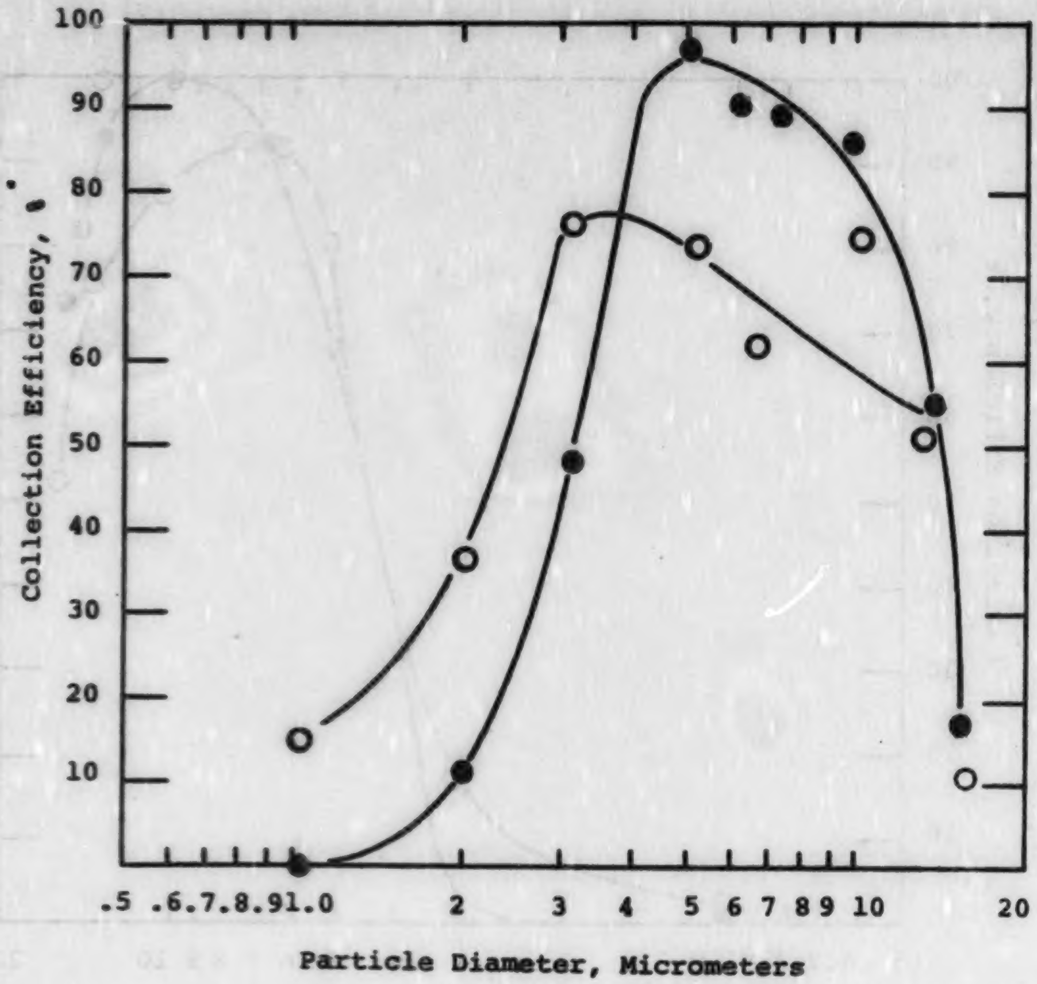


Figure 37. Modified Brink Impactor, glass fiber substrates, stage 2. V_j = 5.8 m/sec, Re = 659, calculated D_{50} = 3.2 μm.

substrate shows a maximum collection efficiency slightly under 80 percent. Also, the D_{50} for the stage which employs the greased foil substrate is near 3.2 μm as given by the manufacturer, while the D_{50} for the glass fiber substrate is moved toward smaller particle sizes somewhat below 3.0 μm.

Figure 38 - Stage 3 - Brink

The trend established in Figure 37 can also be seen in Figure 38. The maximum efficiency for the greased foil substrate is near 100 percent while that for the glass fiber substrate is below 80 percent. The D_{50} for the greased substrate is near that given by the theory of Ranz and Wong and by the manufacturer while the D_{50} for the stage which employs the glass fiber substrate is shifted toward smaller particle sizes and is approximately 1. 5 μm.

Figure 39 - Stage 4 - Brink

This figure shows calibration data for stage 4 for the Brink impactor. Although our aerosol generator was not adequate for good calibration below 1 μm, that is, to determine the D_{50} for these stages, once again it can be seen that the peak efficiency for the greased foil substrate is going to be much higher than that for glass fiber.

Figures 40 and 41 - Wall Losses - Brink

Wall losses as shown for the Brink impactor follow the trends demonstrated in the case of the other impactor calibration studies. There is sufficient scatter in our data that we are not able to determine if wall losses are larger in the case of glass fiber or greased substrates. It is clear, however, that in both cases a significant or large fraction of the total wall losses is due to losses in the nozzle of the impactor.

Figures 42 and 43 - Summary - Brink

Figure 42 shows the graph of collection efficiency vs. square root of Stoke's number for the Brink impactor with glass fiber substrates. It can be seen that the lower stages with the higher jet velocities never attain efficiencies near the ideal value of 100 percent. In fact, stage 6 appears to never achieve efficiencies over 50 percent. It can also be seen that each stage has a characteristic calibration constant which should be used in data reduction to calculate the cut point for that particular stage.

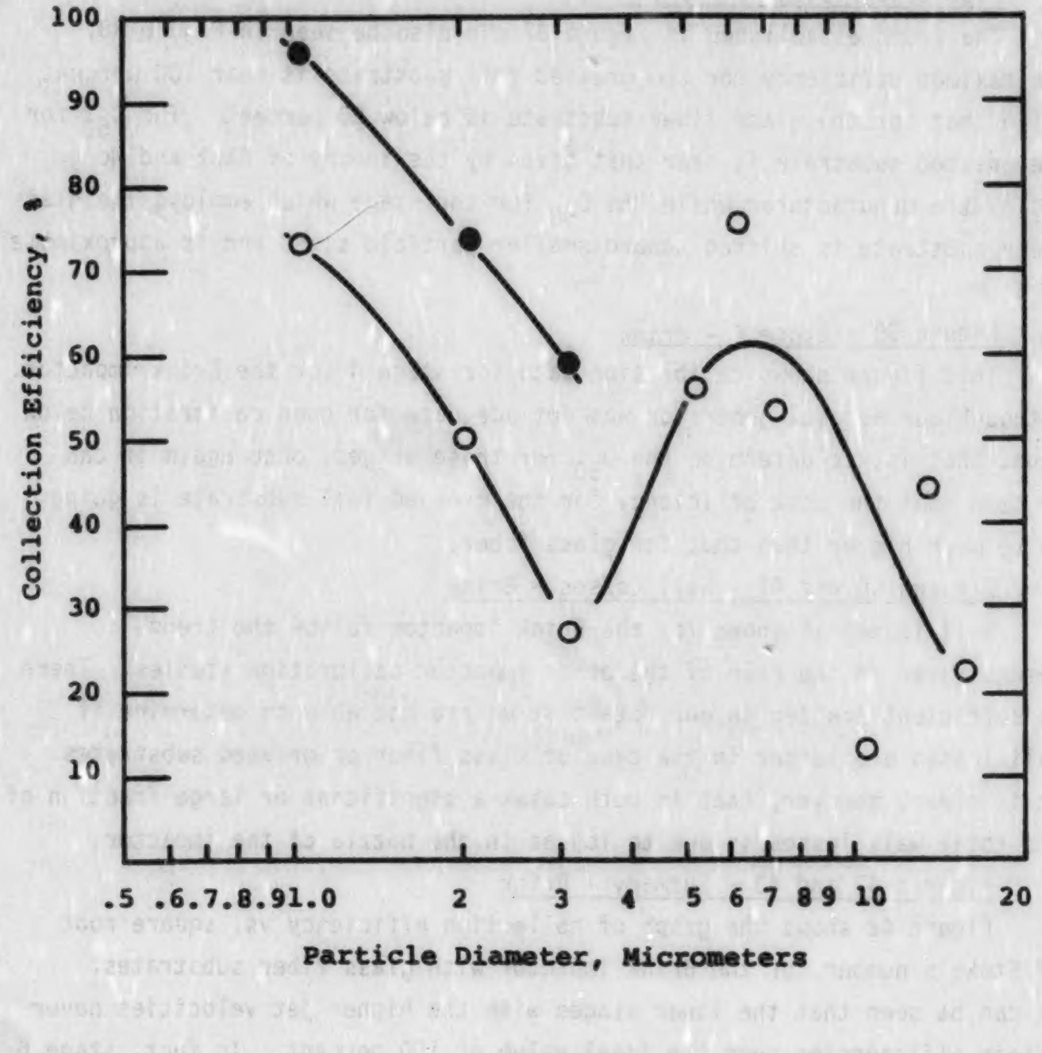


Figure 38. Modified Brink Impactor, glass fiber substrates, stage 3. V_j = 9.2 m/sec, Re = 833, calculated D_{50} = 2.2 μm.

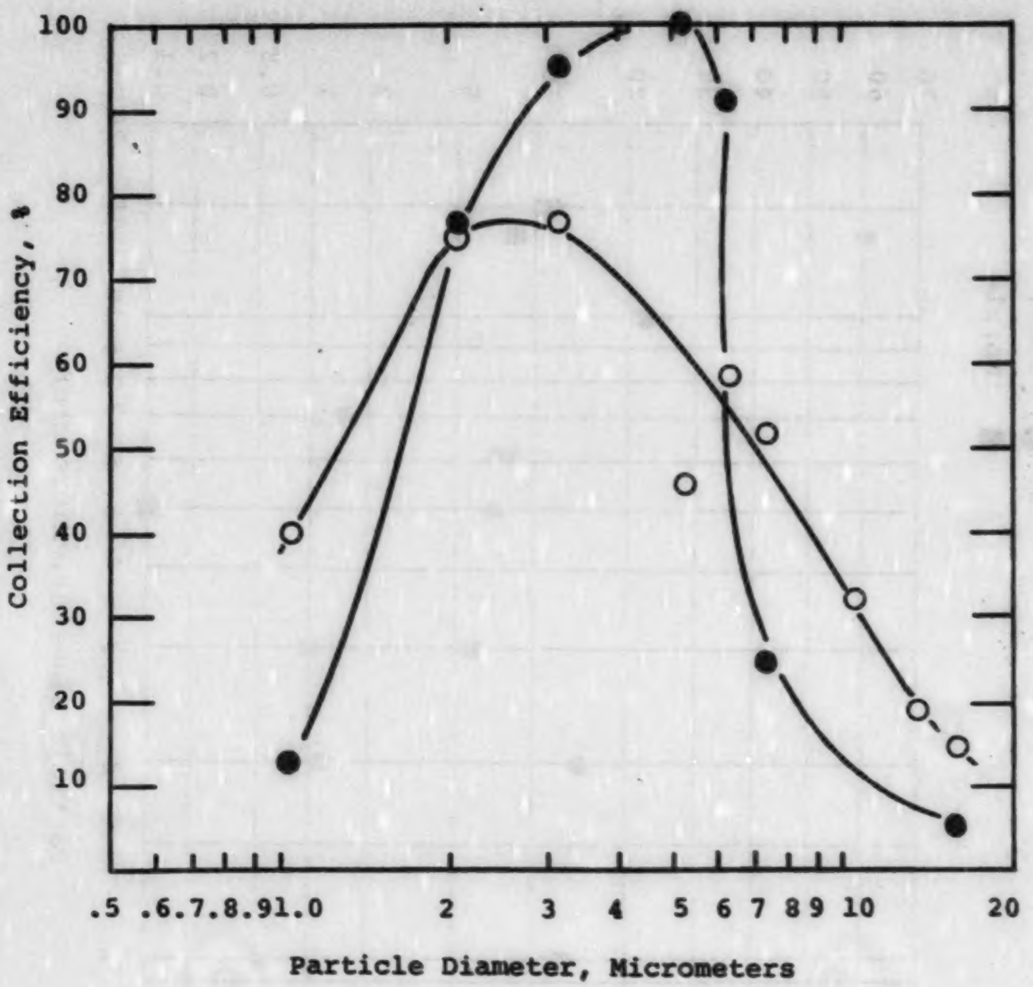


Figure 39. Modified Brink Impactor, glass fiber substrates, stage 4. V_j = 20.3 m/sec, Re = 1236, calculated D_{50} = 1.2 μm.

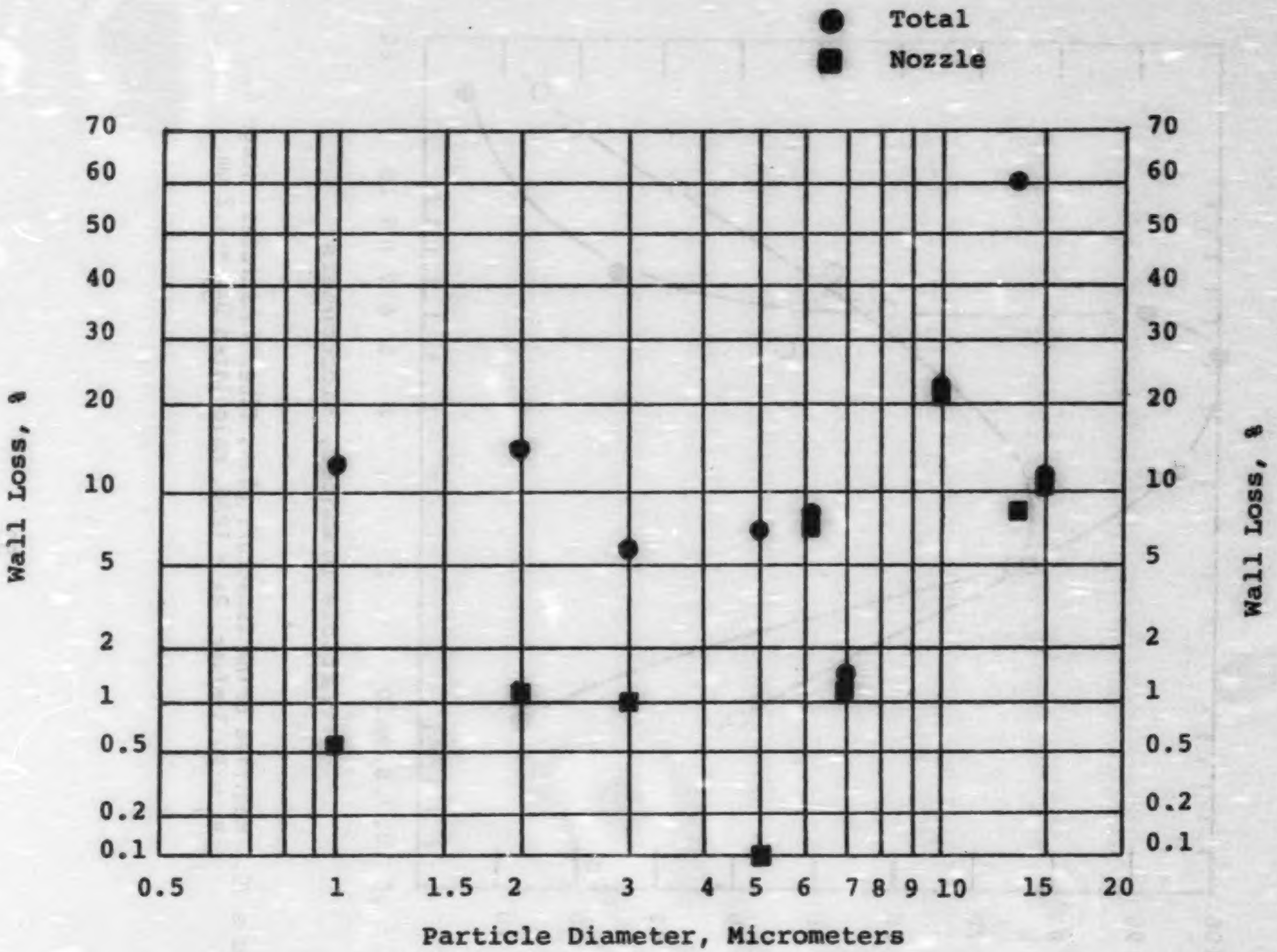


Figure 40. Modified Brink Impactor wall losses, glass fiber substrates.

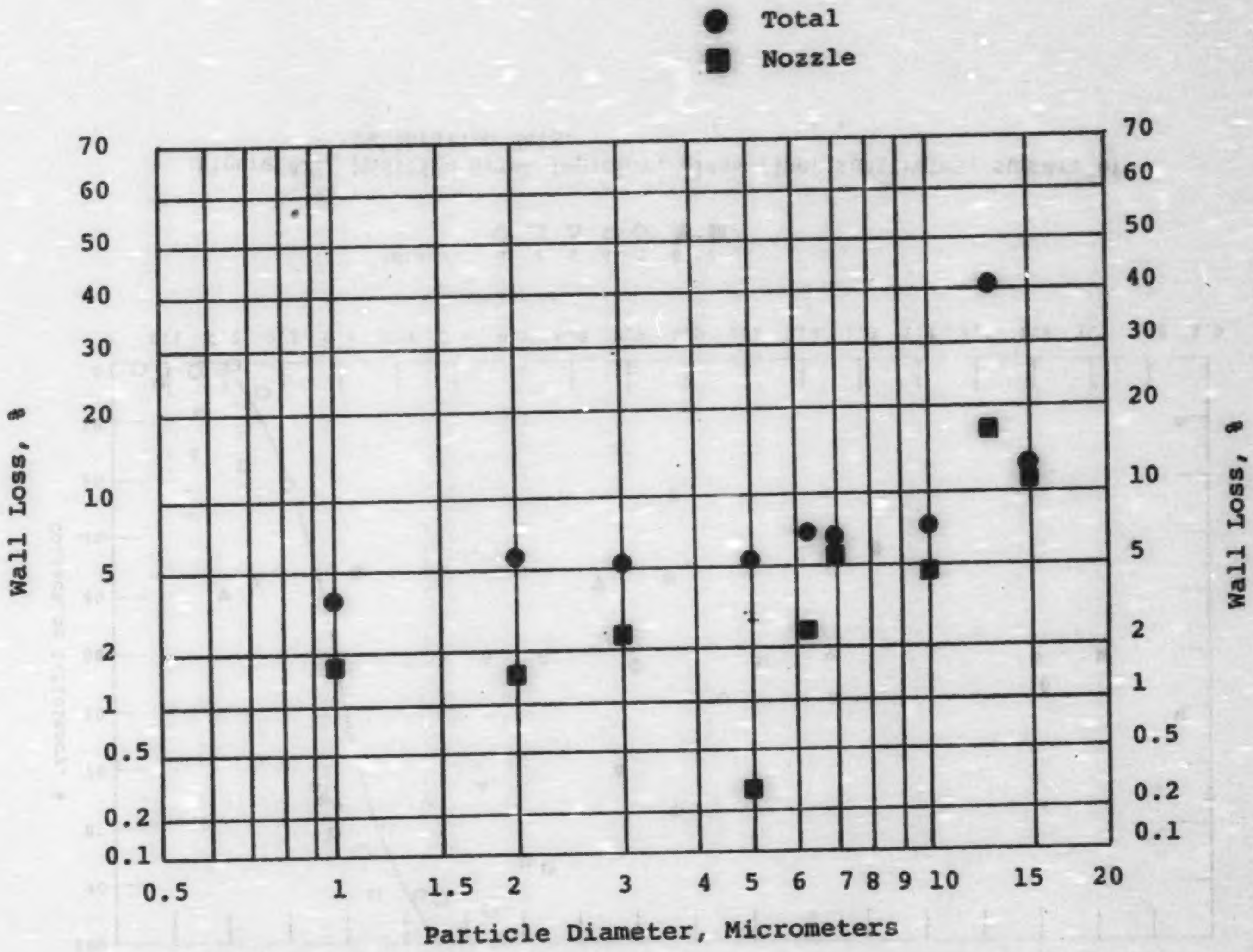


Figure 41. Modified Brink Impactor wall losses, greased substrates.

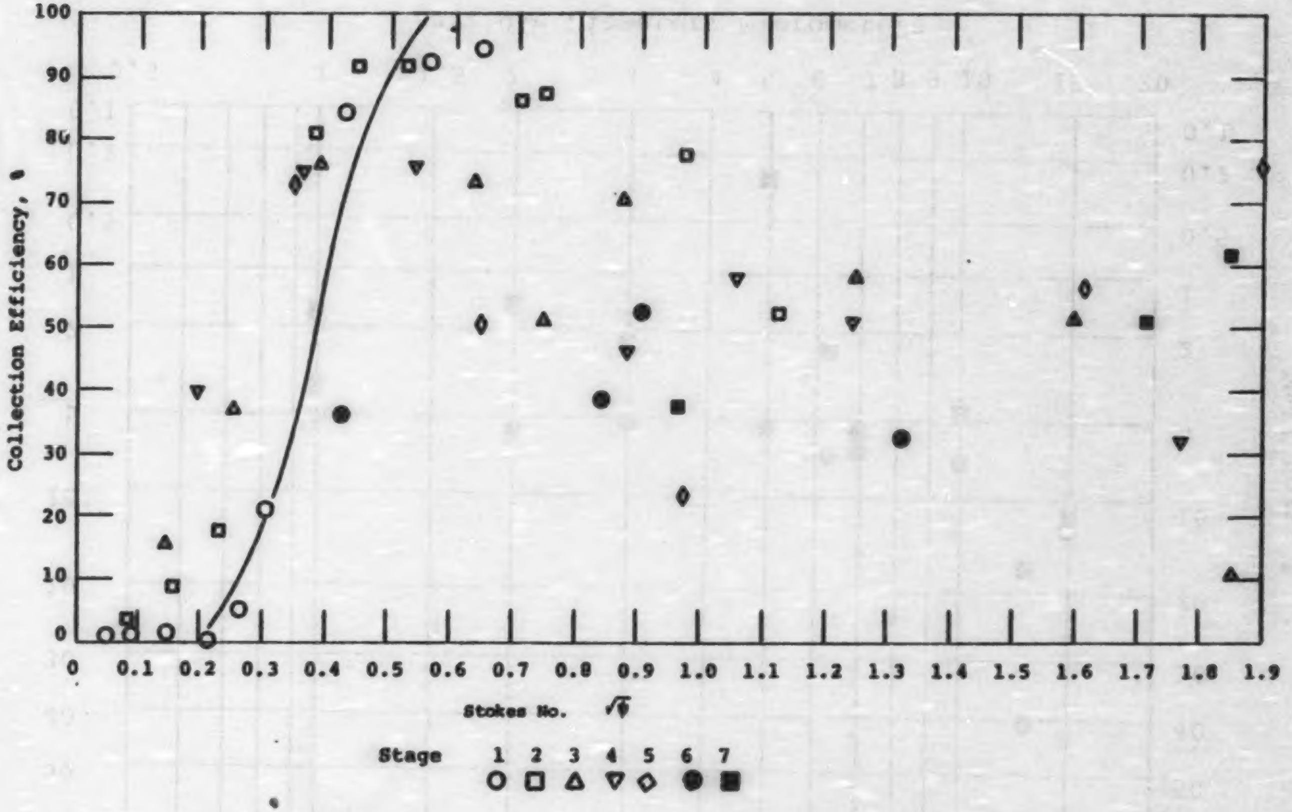


Figure 42. Modified Brink Impactor, glass fiber substrates, summary of calibration data.

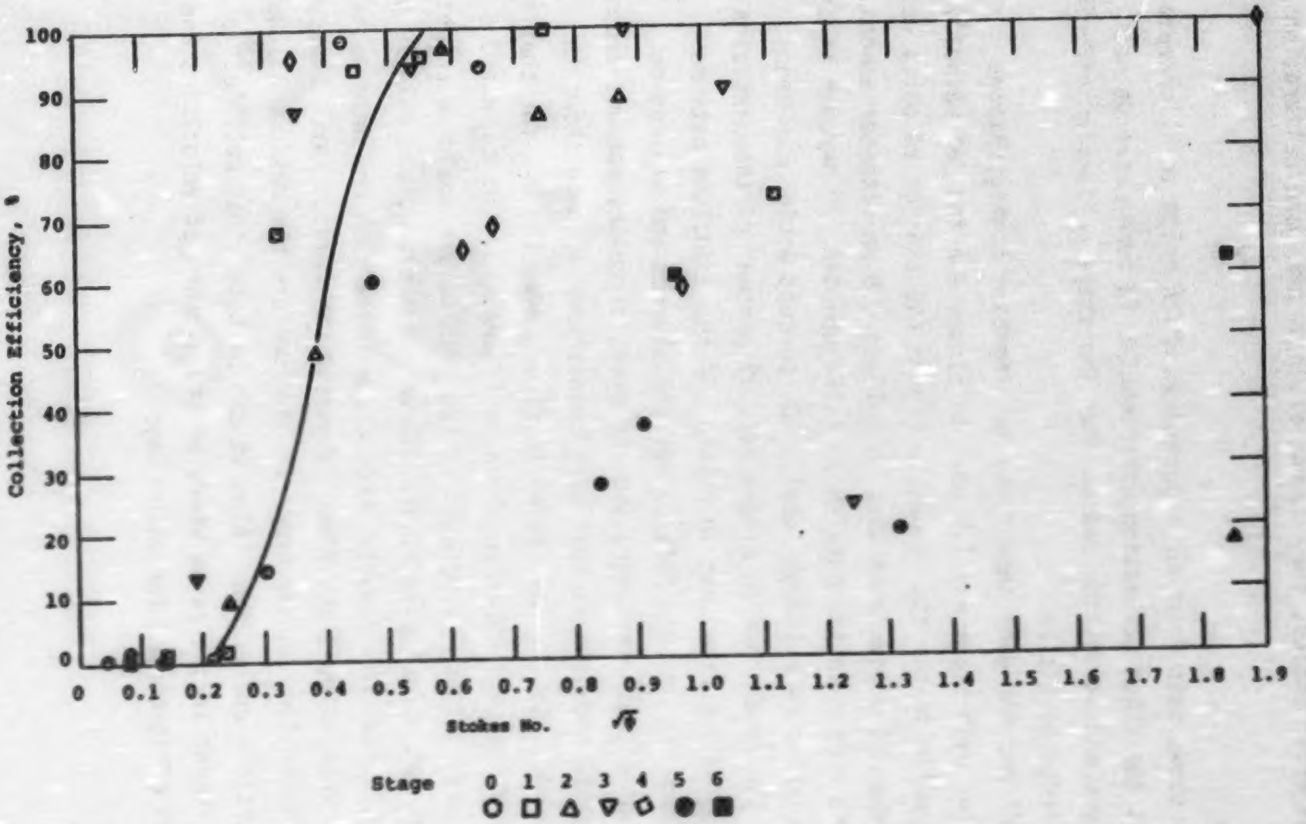


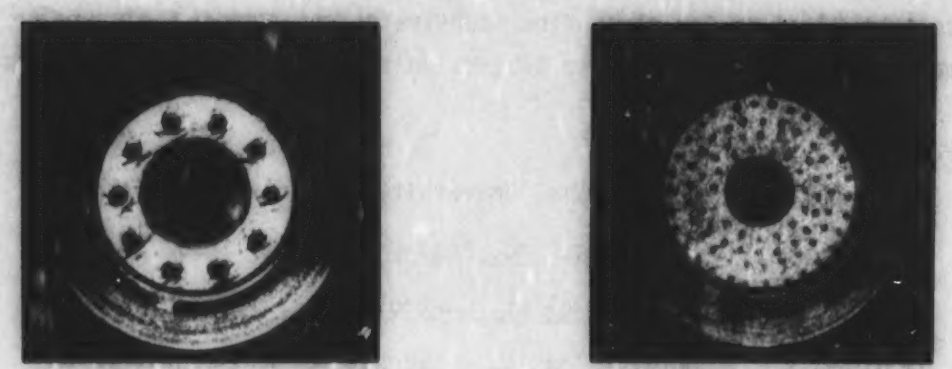
Figure 43. Modified Brink Impactor, greased substrates, summary of calibration data.

Figure 43 shows similar data for the Brink impactor with greased substrates. Although in this case, the stages in general exhibit higher peak collection efficiencies, they do not all have the same calibration constant.

Figure 44

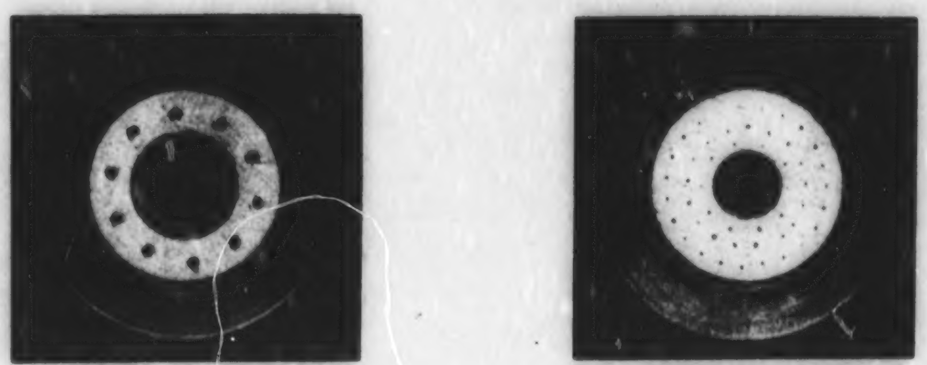
Figure 44 shows data from our laboratory, which helps to illustrate the reason that the stage collection efficiencies in some cases do not reach 100 percent and, also, the reason that the stage collection efficiencies turn down for large particles.

Figures 44A and 44C each show round jet impactor configurations where the D_{50} for each stage was 1.8 μm. In Figure 44A the jet velocity was 11.4 m/sec while in the case shown in Figure 44C the jet velocity was 4.2 m/sec. These jet stages were used to collect 2.8 μm diameter ammonium fluorescein particles. In the case shown in Figure 44A, 73 percent of the particles were collected although ideally 100 percent would have been collected. In the case shown in Figure 44C, 92 percent of the particles were collected. Also notice that in Figure 44A the deposited patterns of particulate are not sharply defined but are blurred and smeared on the substrate while those in Figure 44C are nice, circular, compact deposits. Figures 44B and 44D show stages that were downstream of those shown in Figures 44A and 44C. In the case shown in Figure 44B, the D_{50} for that stage was 0.38 μm. For the configuration shown in Figure 44D, the D_{50} was 0.8 μm. Thus, we would expect that 100 percent of the particulate would be caught in both cases. The jet velocity in Figure 44B was 45.1 m/sec, and only 79 percent of the particulate was caught. From experiments such as these we have concluded that, when using glass fiber substrates to collect hard, bouncy particles, jet velocities on the order of 10 m/sec are the maximum allowable for adequate particle collection. Also it can be noted that wall losses were much more significant in the cases where we had higher jet velocities and lower collection efficiencies for each stage.



a. V_j = 11.4 m/sec, 73% COLLECTION

b. V_j = 45.1 m/sec, 79% COLLECTION. 32% WALL LOSSES IN a AND b



c. V_j = 4.2 m/sec, 92% COLLECTION

d. V_j = 9.5 m/sec, 94% COLLECTION. 4% WALL LOSSES IN c AND d

Figure 44. Particulate deposition patterns for different flow rates. In all cases the particles were 2.8 μm diameter ammonium fluorescein spheres. a. D_{50}=1.8 μm, b. D_{50}=0.83 μm, c. D_{50}=1.8 μm, d. D_{50}=0.38 μm.

The data presented above show that impactors should actually be calibrated to determine the cut points, rather than relying on theoretical predictions. Maximum allowable jet velocities appear to be less than 20 m/sec for glass fiber substrates and somewhat higher for greased substrates. This also depends on the type of particulate sampled.

References

1. Rao, A.K., Doctoral Thesis, University of Minnesota, 1975.
2. Ranz, W.E. and J.B. Wong, Ind. Eng. Chem. 44(6), 1952.
3. Lundgren, D.A., J. Air Pollut. Control Assoc. 16, 225, 1967.
4. Marple, V.A., Doctoral Thesis, University of Minnesota, 1970.

DISCUSSION

EMSOR: Did you measure the actual hole diameters in the impactors. We did some analysis last year which indicates that the cut is very sensitive to the diameter of the holes. Sometimes the manufacturing can be off and can cause some shifts in efficiency.

SMITH: In some of the impactors we did. We measured them on the Russian impactor. I believe we measured them all. Do you remember, Joe?

MCCAIN: We measured the Brink, the Andersen, University of Washington, the Russian. I don't know whether we measured them on the MRI.

SMITH: In most cases, they showed less than 10 percent error in diameter.

RAO: How did you measure the hole diameter?

SMITH: With an image-shearing eyepiece on a microscope.

RAO: We found that, if you measure the diameter by pressure drop, you can get much more accurate measurements.

SMITH: More accurate measure or more predictable performance?

RAO: Yes, it's a good measure--more accurate.

SMITH: We found what we thought was so little error, although there is a very strong dependence on jet diameter, that we didn't do anything with it. For example, we didn't feel that we needed to correct any of the calculated D_{50}'s because of the small errors that we found.

BLANN: Is this roll over in this curve something new? Or has this been seen before?

SMITH: We published some data to that effect in the Report 21 of ours to EPA, which got considerable circulation. I don't know how much was in the literature before that. That was in 1972. It's not surprising, when you think about the details of what is going on. The larger particles hit with a lot of momentum and bounce. I think it has become commonplace knowledge in the last year or so.

MCCAIN: The expectation for that kind of behavior was realized before we first started this stuff. Some people said when we first started impactor work; "It'll never work, look at a sandblaster." The thing that's remarkable here is that they've been any good.

_____: You made the comment several times, "This is a well-behaved stage." Well, define well-behaved stage.

SMITH: By that I mean that it went up to near 100 percent (collection) and then collected essentially 100 percent of the larger particles over a pretty broad range. So if upstream from that, you have a stage with a D_{50}, say, of twice that D_{50}, that stage will collect any particles of a size that would cause the efficiency to roll back down. If you have got a sharply peaked stage efficiency, then you must have another stage upstream with a slightly larger D_{50}. If you overlay the stage efficiencies, you would hope that, before one turns down to large particles, the other would pick up and be collecting large particles. The perfect stage would be one that goes up to 100 percent and stays there for all large particle sizes.

BOLL: I wonder if you could tell me what your particle material was and would you elaborate on why the bare metal and the Teflon were totally inadequate.

SMITH: The material was ammonium fluorescein, and we picked that material because of a paper that was published. I can't remember the author's name.

RAO: Stober and Flachsbart.

SMITH: That's right. It is nonhydroscopic, whereas ordinary Uranine is hydroscopic, and we wanted a material that was bouncy to simulate fly ash. There have been studies similar to this done with oils, and in those cases glass fiber, bare metal, Teflon, and certain materials worked well as substrate materials. Most of our work is done at power plants, and fly ash is a bouncy material. We wanted to simulate that behavior. In the case of Teflon, the material doesn't deposit right under the jet; it just blows around and settles where there are eddies on the surfaces, and much of it ends up downstream on the backup filter. Essentially, the same thing happens with bare metal when you have a bouncy material; it just blows right off.

BOLL: Then your basis for saying that is observing a dune structure on substrate.

SMITH: Right, and if the stage had a predicted efficiency of 100 percent and we got 10 percent, we would say that that's a reason for not using it also. The material just blows off, and a lot of it ends up downstream.

OLIN: Wallace, you plotted the collection efficiency of the incident aerosol.

SMITH: That's right. The collection efficiency was calculated from a knowledge of the amount of material which was captured on a given stage and of the amount of material which penetrated that stage.

OLIN: So, therefore, if the preceding had been a normal impactor acting in the cascade configuration, many of these particles that do bounce and are shown on your efficiency curves would be removed before they got there.

SMITH: Some of them would be removed before they got there.

OLIN: Yes, some of them.

SMITH: These are curves for a particular stage and actually the efficiency of the stage doesn't enter into the data reduction as it is done; normally, you just put in the D_{50}. The assumption is that above a certain size, 100 percent are caught. I'm not quite sure what effect this has on the calculated size distribution, certainly it would shift.

OLIN: So if your first stage is quite efficient, then most of these impactors did show that you've been removing a good portion of those large particles or if you had a preseparator, such as cyclone preseparator, you would be removing those particles before they had an opportunity to bounce.

SMITH: Well, that is sort of true, but there is a size at which the first stage has zero collection efficiency and it may be like 5 or 6 microns, so those particular sizes would reach the downstream stages where they must be caught.

HARRIS: The problem is that each one of these stages has an efficiency. It's a problem then of spacing the cut points to that a stage upstream of it is collecting 50 percent of the particles but the other stages are collecting 0 percent of this. That means 50 percent of the particles are going to go flying through.

SMITH: The problem is that large particles which penetrate past the stage where you would like them to be caught are on the negative slop of the efficiency curve of downstream stages, and there is a decreasing probability that they will be caught as they go downstream. Thus, they may well end up on the backup filter, where a single large particle can be counted as a large number of small particles.

SPARKS: In analyzing these stages, stage by stage, this is fine, but have you done any work where you put in a known size distribution with an assembled impactor stages and came out with results on the size distribution as you interpret it and relate that to what you expect.

SMITH: The impactors were all run completely assembled; i.e., we did not disassemble any to do the analysis stage by stage. They were operated as a whole and the analysis was done stage by stage. However, the size distributions were essentially all monodisperse. We never had a polydisperse aerosol of known size distribution.

_____: You haven't tried to get the empirical data on known size distributions; see what the results would be on the basis of analysis of that?

SMITH: Right, we have done a little bit of that theoretically, and we also compared the performance of different impactors in the field.

HARRIS: We've done the same thing on the wind tunnel; Bill [Kuykendal] attempted that.

DICK: Could you tell us how much material you were depositing per jet?

SMITH: Well, I really can't. In some cases, with the larger particles it is fairly easy to deposit a total of approximately a milligram. For the smaller particles, the analysis by washing the impactor and determining the mass was done with absorption spectroscopy. We were specifically avoiding the very long run times necessary to deposit enough material so that we could weigh it.

The amount of bounce you can get probably depends on how much you put down.

SMITH: I think it definitely does.

MCCAIN: The deposits were always several particle layers thick. I don't think there was anything that was done in which the deposit was essentially monolayer.

SMITH: For example, in the case of tests using very small particles would run the impactors for two days to catch enough material for analysis. But, as I said in my opening remarks, Lundgren has shown that the collection efficiency is a function of time just as you suggested. He showed curves with grease substrates where the efficiency changed, I believe, from around 90 percent to 70 percent over a 20 to 30 minute period.

SUBSTRATES AND IMPACTOR CUT POINTS

A. Kishan Rao, Midwest Research Institute

RAO: Primary fine particulate emissions from industrial sources are one of the EPA's major control targets. For this reason, EPA is interested in characterizing particulate emissions from various sources and in evaluating emission control devices. Cascade impactors of various designs have been used for a number of years for physical and chemical characterization of aerosols. They are well suited for in situ source testing because of their simple construction and operation, compactness, ability to withstand high temperatures and pressures, and perhaps most importantly, because they classify particles according to their aerodynamic size. Additionally, they have much better size separation properties than competing techniques (e.g., cyclone separators). Impactors, when used properly, can provide reliable particle size distribution data.

Because of their wide use, impactors are the subject of considerable theoretical and experimental investigation. Ranz and Wong[1] developed a simple theory in 1952, which is generally used in predicting impactor performance. Recently, Marple et al.[2,3] developed a numerical calculation procedure more accurate than the Ranz and Wong theory. These theories however, are limited to impactors of simple geometry operating under idealized conditions. It will be shown in this paper that, for impactors such as the Andersen sampler, these theories are inadequate, and experimental calibrations are essential if reliable data are to be obtained.

It is well known that solid particles may not stick to collection surfaces unless the collection surfaces are coated with some adhesive. In the event of particle bounce, reentrainment, and deagglomeration or breakup, the impactor loses its ability to classify particles according to size, resulting in erroneous and misleading size distribution data. The conventional method of reducing particle bounce and reentrainment is by coating the collection surface with viscous oils or with other adhesive materials. However, when samples are to be analyzed for chemical composition, use of adhesive coatings is not recommended for fear of sample contamination and possible interference with analysis techniques. In recent years, fibrous filters have gained wide acceptance as collection surfaces because they facilitate sample handling and appear to reduce particle bounce.[4] However, the real collection characteristics of these filters had not been investigated prior to this study.

This paper presents the main results of an experimental study which was aimed at defining some of the nonideal collection characteristics of inertial impactors. Specifically, the problems of particle bounce and "blow-off", or reentrainment, were studied.

In this study, two single-stage and two cascade impactors were used. Figure 1 shows a schematic diagram of the laboratory impactor. The jet-throat has a 60° conical entrance. The jet-to-plate distance is approximately one jet diameter. Figure 2 shows a schematic diagram of the Bureau of Mines impactor. The design of this impactor is similar to the laboratory impactor with the exception of jet-to-plate distance, which in this case, is 3.44 times the jet diameter. The Lundgren impactor shown in Figure 3 is a four stage, rectangular jet impactor, and it is designed such that all of its stages are geometrically and dynamically similar. The Andersen sampler shown in Figure 4 is a six stage, cylindrical, multihole impactor designed for atmospheric sampling.

Figure 5 shows some of the equipment used in this study. The vibrating orifice aerosol generator and the polystyrene latex (PSL) aerosol generator were used to produce the test aerosols. Precautions were taken to produce monodisperse aerosols of spherical, dry, and electrically neutral particles. Four types of collection surfaces were tested. They were: (a) oil-coated smooth surface (glass plate in the case of single stage impactors and stainless steel plates in the case of the Andersen sampler); (b) uncoated surfaces; (c) Gelman Type A glass fiber filter; and (d) Whatman No. 41 filter paper. The oil was either Dow Corning 2000 fluid (with 60,000 cs viscosity) or petroleum jelly.

The collection efficiency curves of single-stage impactors were determined as follows. The PSL aerosols were generated and were sampled at a known flow rate with the test impactor, which was preassembled with one of the collection surfaces to be tested. Collection efficiency was determined

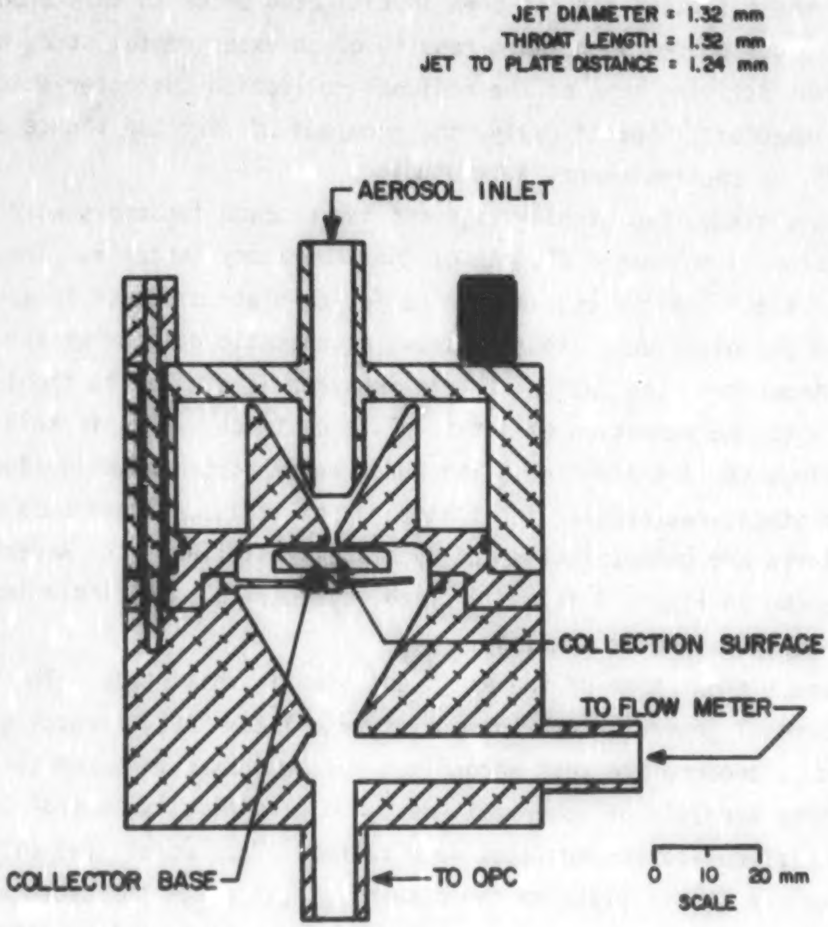

JET DIAMETER = 1.32 mm
THROAT LENGTH = 1.32 mm
JET TO PLATE DISTANCE = 1.24 mm
AEROSOL INLET
COLLECTION SURFACE
TO FLOW METER
COLLECTOR BASE
TO OPC
0 10 20 mm
SCALE

Figure 1. Laboratory impactor.

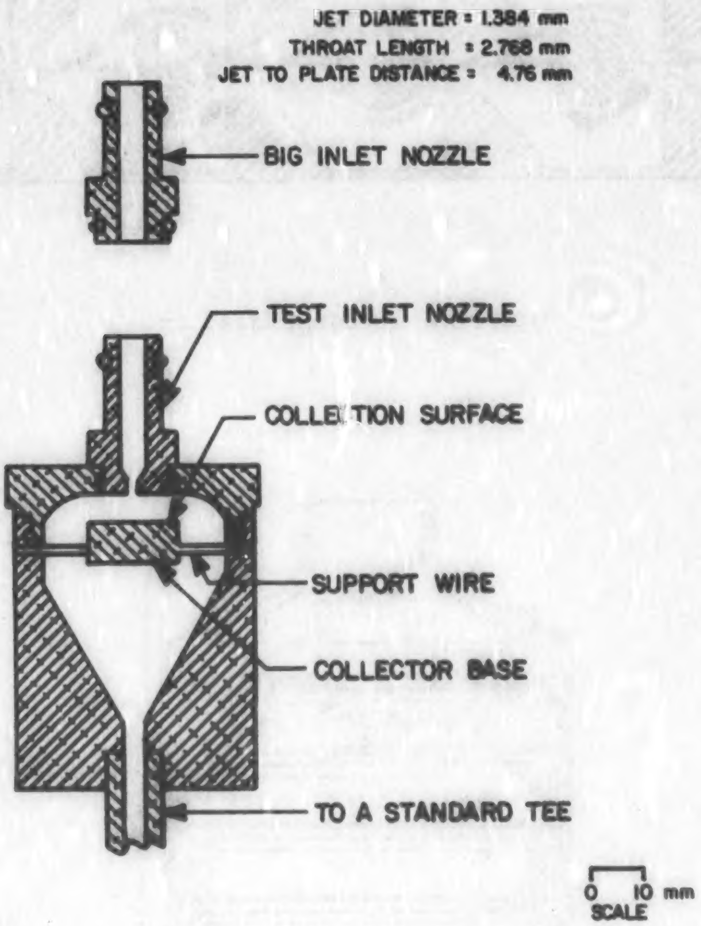

JET DIAMETER = 1.384 mm
THROAT LENGTH = 2.768 mm
JET TO PLATE DISTANCE = 4.76 mm
BIG INLET NOZZLE
TEST INLET NOZZLE
COLLECTION SURFACE
SUPPORT WIRE
COLLECTOR BASE
TO A STANDARD TEE
0 10 mm
SCALE

Figure 2. U.S. Bureau of Mines Impactor.

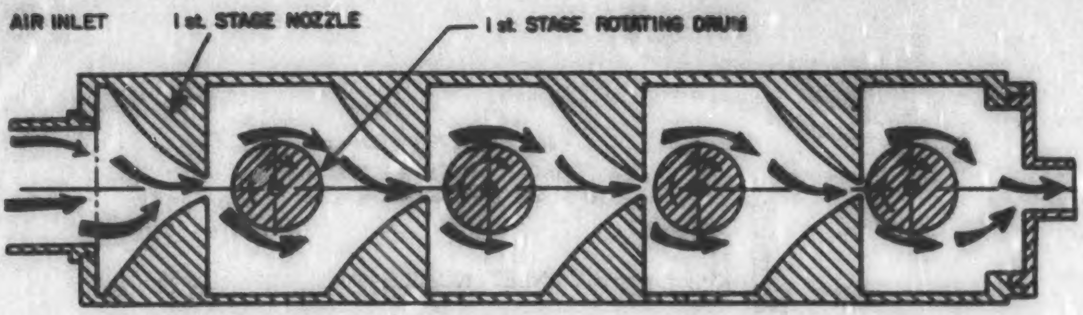

AIR INLET
I st. STAGE NOZZLE
I st. STAGE ROTATING DRUM

Figure 3. Lundgren Impactor.

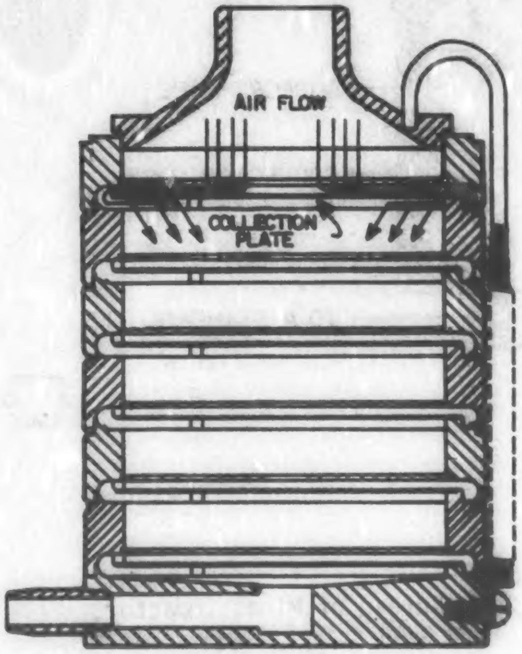

AIR FLOW
COLLECTION PLATE

Figure 4. Andersen Sampler.

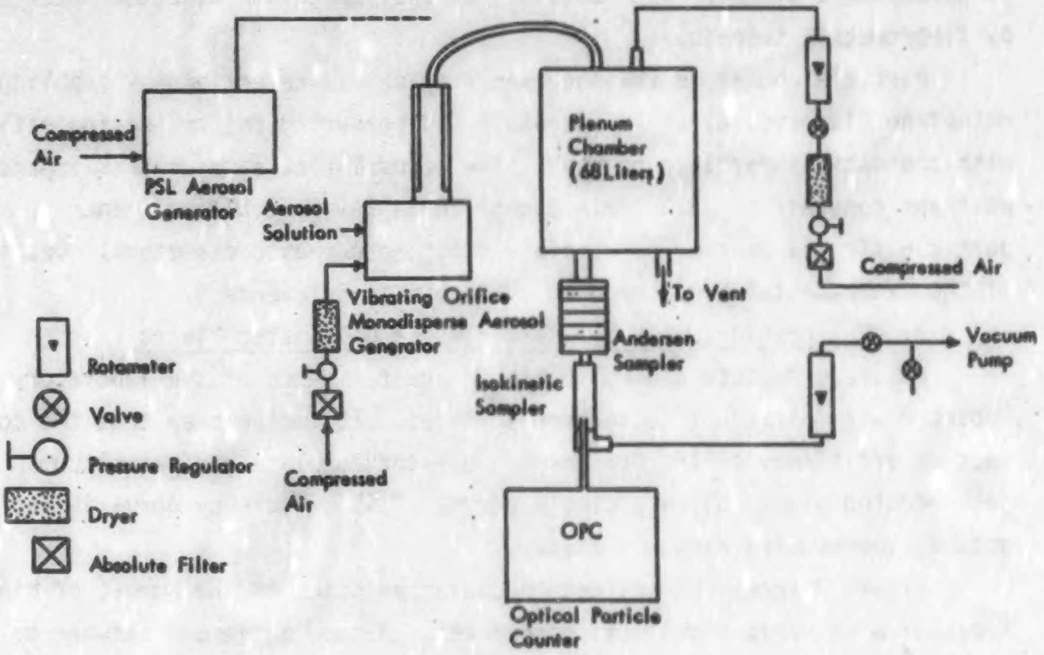

Compressed Air
PSL Aerosol Generator
Aerosol Solution
Vibrating Orifice Monodisperse Aerosol Generator
Compressed Air
Plenum Chamber (68Liters)
To Vent
Compressed Air
Andersen Sampler
Isokinetic Sampler
Vacuum Pump
OPC
Optical Particle Counter
Rotameter
Valve
Pressure Regulator
Dryer
Absolute Filter

Figure 5. Schematic diagram of an experimental set up.

by measuring the aerosol concentration upstream and downstream of the impactor with an optical particle counter. Stokes number was varied by varying the flow rate through the impactor for a given size particle.

The Lundren impactor and the Andersen sampler were calibrated concurrently with oleic acid particles tagged with uranine dye tracer. The flow rates through these impactors were 112.3 and 28.3 l/min, respectively. The collection efficiency and wall loss of each stage as a function of aerodynamic diameter were obtained by analyzing the collected material by fluorometric techniques.

Particle bounce in the Andersen sampler was determined by sampling methylene blue (solid) or PSL aerosols and measuring the collection efficiency with the optical particle counter. The aerosol flow rate of this impactor was kept constant at 28.3 l/min except for a few special cases and particle size was varied to obtain various aerodynamic diameters. Details of the experimental procedure are available in Reference 5.

A. Calibrations with Oil Particles or Oil Coated Plates

Figure 6 depicts the collection characteristics of the laboratory impactor with different collection surfaces. It can be seen that the collection efficiency of the Dow Corning oil-coated plate and petrolatum jelly-coated plate follow a single curve. This efficiency curve is in good agreement with Marple's theory.

Figure 7 shows the collection characteristics of the Bureau of Mines impactor with various collection surfaces. A good agreement between oil-coated plate efficiency and theoretical efficiency is evident.

Figure 8 presents the experimental results for the Lundgren impactor operating at a constant flow rate of 112.3 l/min. For comparison also shown in this figure are Marple's theoretical curve and Lundren's experimental curve published previously.[6] Theoretically, if the stages of an impactor are geometrically and dynamically similar, their collection efficiency curves should be similar. For such an impactor, the collection efficiency curves of different states (i.e., efficiency plotted versus the square root of the Stokes number) should fall on a single curve. The present data adequately define a smooth "S" shaped curve which is in good

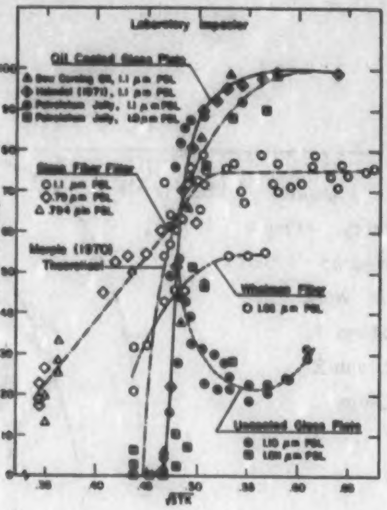


Figure 6. Collection characteristics of the laboratory impactor.

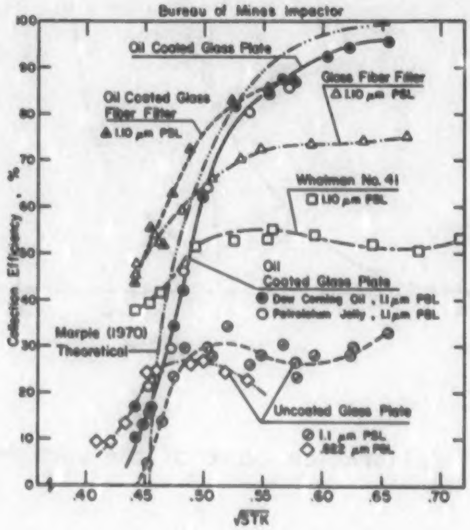


Figure 7. Collection characteristics of the Bureau of Mines Impactor.

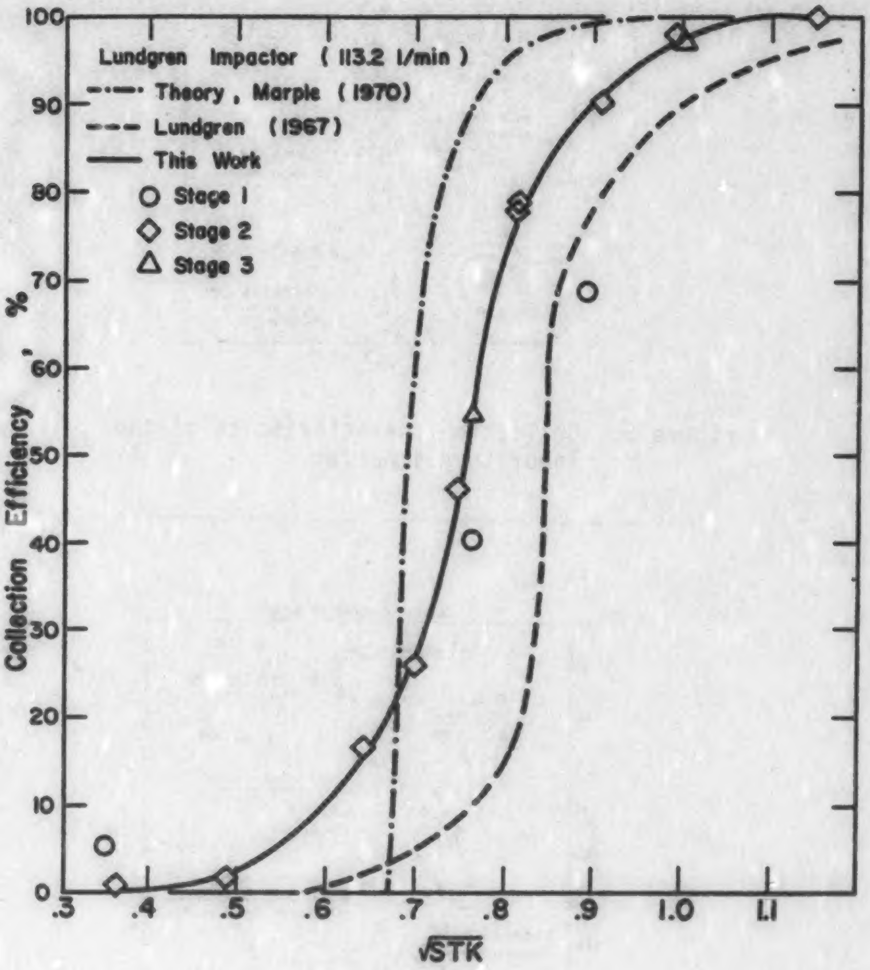


Figure 8. Calibration curve of the Lundgren Impactor.

agreement with both theoretical efficiency curves and Lundgren's experimental curve. This agreement between theoretical predictions and experimental calibrations indicates that the theory can accurately predict the performance of impactors of this type.

Figure 9 illustrates the collection efficiency curves of the Andersen sampler operating at a flow rate of 28.3 l/min. These calibration curves were obtained with oil aerosols and stainless steel collection plates. For comparison Marple's theoretical curve corresponding to a jet Reynolds number of 3000 and jet-to-plate distance of five jet diameters is also plotted in Figure 9. It can be seen that the efficiency curves of different stages have different slopes and the curves do not coincide. The experimental efficiency curves have smaller slopes and are shifted towards higher Stokes numbers relative to the theoretical efficiency curve. This result means that for this type of impactor theoretical prediction of the performance would result in considerable error, and experimental calibrations are essential if reliable data are to be obtained.

The reasons for disagreement between the theory and experimental calibrations of the Andersen sampler are the following:

1. The theoretical curve is developed for a simple geometry impactor such as a single jet impactor with jet-throat having a conical entrance and operating at a jet Reynolds number of 3000. The Andersen sampler is a multijet, straight-hole impactor with jet Reynolds numbers between 84 and 400.

2. In the case of multijet impactors, it is usually assumed that flow through each jet is equal. However, in the Andersen sampler this assumption does not seem true, especially for Stages 1 and 2.

The effectiveness of an adhesive coating to prevent bounce may decrease with stage loading. To investigate this possibility, several tests were performed with the Bureau of Mines impactor. Because of the large jet-to-plate distance of this impactor (3.44 jet diameters) slight changes in the jet-to-plate distance with load were not expected to affect its performance. The impactor plate was coated with a thick layer of petrolatum jelly and 1.1 μm PSL aerosol was used as the test aerosol. The results are shown in Figure 10. In this figure, we see no indication of bounce with loading. However, the overloaded surface has a much higher collection efficiency than the fresh surface.

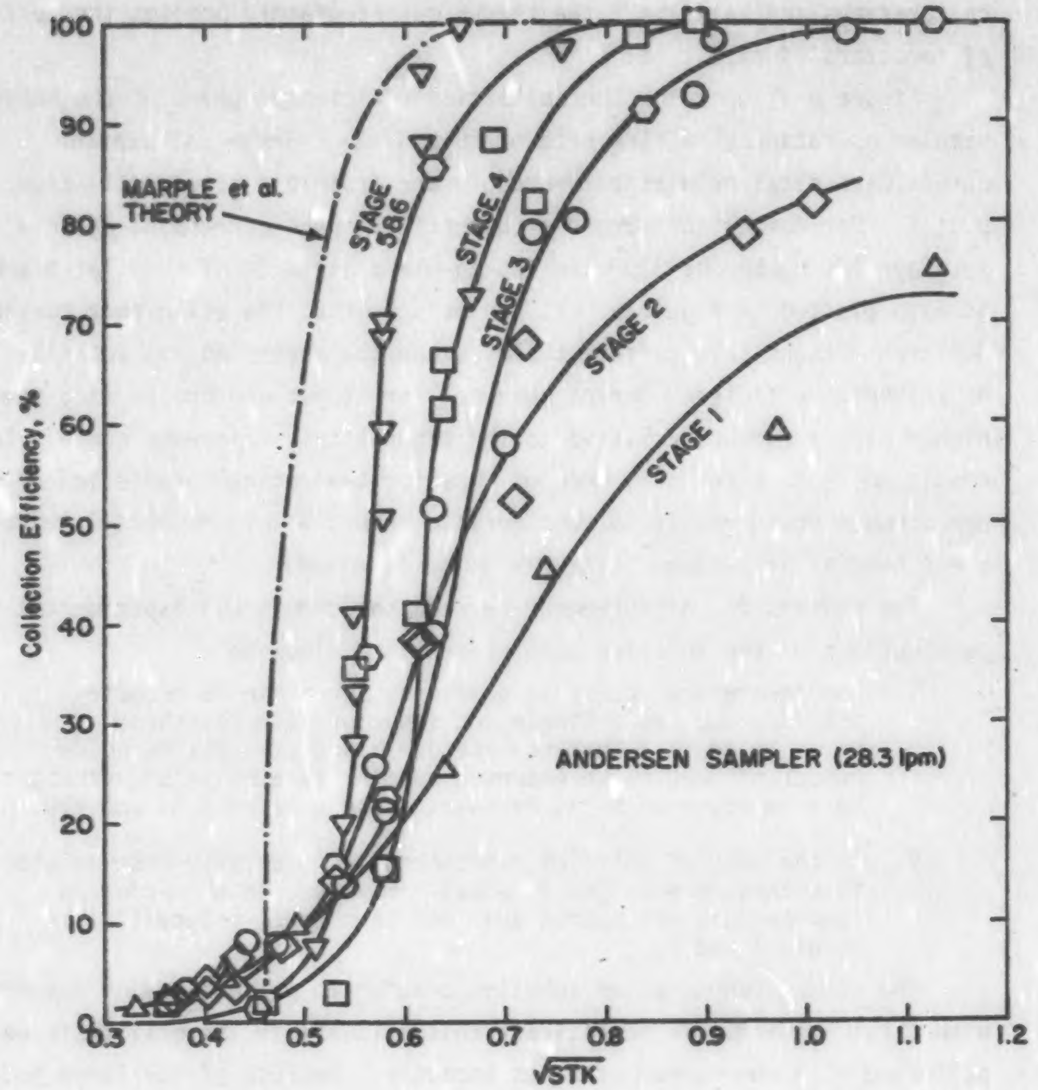


Figure 9. Calibration curves of the Andersen Sampler.

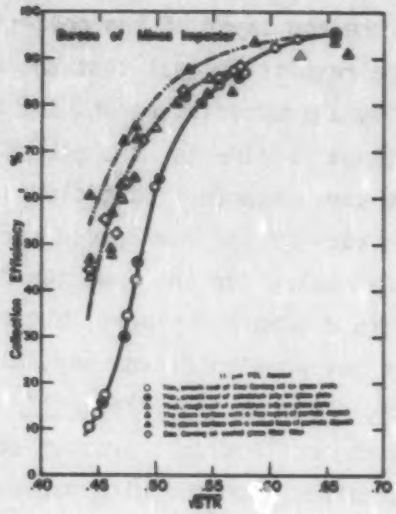


Figure 10. Effect of Load on the Impactor Performance

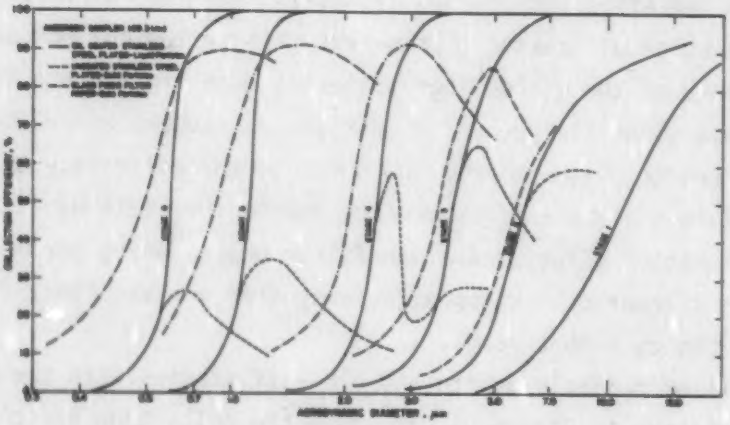


Figure 11. Collection Efficiency of the Andersen Sampler with Various Collection Surfaces

On examination of the overloaded surface it was found that the adhesive actually rose to the top layer of the collected particles by surface tension forces. These results suggest that the adhesive selected should have good wettability characteristics and the aerosol desposition rate should be such that there is time for the adhesive to rise up to the precipitate surface before any subsequent particle impingement. The increased collection efficiency of the overloaded surface also suggests that a correction may be necessary for the impactor constants if they were originally obtained with fresh smooth surfaces, but are actually used with rough surfaces or surfaces having significant particle loading.

B. Analysis of Particle Bounce and "Blow-Off"

Figures 6 and 7 show the collection characteristics of the laboratory impactor and the Bureau of Mines impactor with various collection surfaces. In these figures, we see that for solid particles the collection efficiency curves of the uncoated glass plate follow the efficiency curves of the oil-coated plates only up to 20 to 50 percent efficiency. At higher Stokes numbers, collection efficiency curves of the uncoated glass plate drop sharply.

Collection efficiency curves of filter surfaces are qualitatively different from the efficiency curves of oil-coated glass plates. At low Stokes numbers for which oil-coated plates have zero or low collection efficiency, the efficiency of the glass fiber filter is much higher than the efficiency of oil-coated glass plates. At higher Stokes numbers where the efficiency of the oil-coated plate is near 100 percent, the efficiency of glass fiber filters attain a plateau of 75 percent due to the limiting effects of the surface filtration effect. Whatman filter paper, being harder and less porous, has a lower collection efficiency than a glass fiber filter over the entire Stokes number range.

Results of particle bounce and blow-off studies with the Andersen sampler are shown in Figure 11, in which the collection efficiency as a function of aerodynamic diameter is plotted for each stage with oil-coated stainless steel plates, uncoated stainless steel plates, and the glass fiber filter. The data points used to draw each of these curves are not shown for the sake of clarity. The shape of the curves in this figure is similar to the shape of the curves in Figures 6 and 7 adding to the generality of the results described earlier.

Figure 11 shows that particle bounce with uncoated plates can significantly lower the collection efficiency and is especially serious for stages which collect small particles at high jet velocities. It appears that, in the presence of bounce, efficiency is not controlled by Stokes numbers alone. Particle size, jet velocity, and other factors affecting particle adhesion also affect the collection efficiency. Glass-fiber-filter collection surfaces reduce particle bounce significantly, but change the characteristic curves in such a way as to reduce the size resolution of the impactor. Compared to a smooth surface, the glass fiber filter becomes increasingly efficient as the particle size decreases. This improvement is due to the changes in the relative roughness of the filter surfaces as can be seen in Figure 12. For upper stages which collect larger particles, the relative roughness of the filter surface is low and the filter behaves very much like a smooth surface. However, for lower stages which collect relatively smaller particles, the fibers and the void spaces of filters are big compared to the particles, and the particles have a greater chance of being collected when moving close to the filter surface.

The following conclusions can be drawn:

1. For impactors such as the Andersen sampler (six stage, ambient type) whose design and operation do not satisfy all the assumptions of the Marple theory, the theory cannot predict the performance. The experimental stage cut-off diameters of the Andersen sampler are significantly different from the theoretical stage cut-off diameter. This result suggests that using the Ranz and Wong theory or the Marple theory to obtain stage cut-off diameters of these impactors for calculating the size distributions results in considerable error.
2. The performance of an impactor is significantly affected by the nature of its collection surface. Glass fiber filters, generally used to reduce particle bounce, shift the collection efficiency curves and decrease the sharpness of cut. The magnitude of the shift is different for different stages. This result suggests the need for calibrating the impactors under conditions in which they are used.

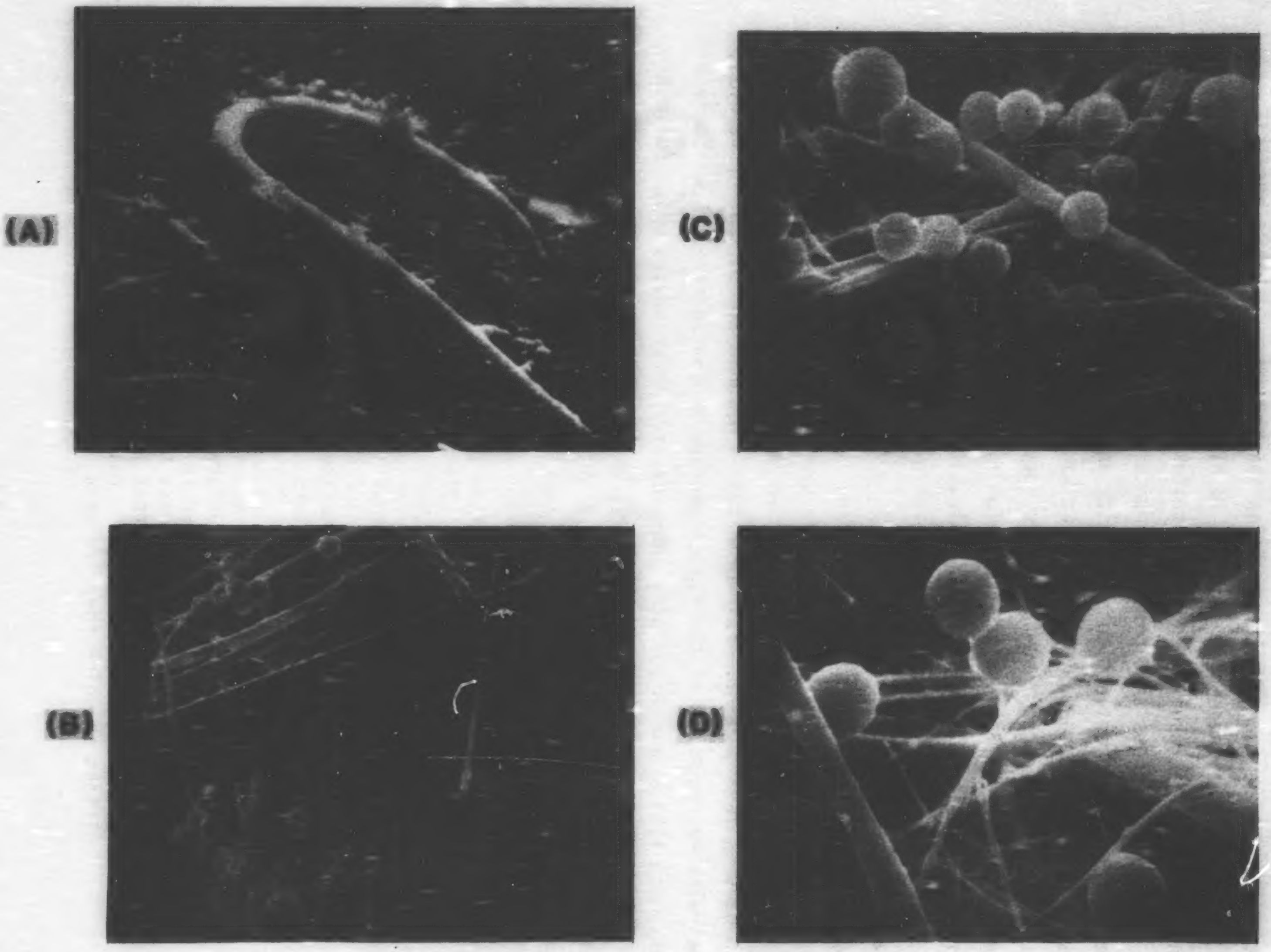


Figure 12. Electron micrographs of the glass fiber filter surface showing the relative size of particles and fibers. (a) 0.481 μm PSL; (b) 2.75 μm methylene blue; (c) 3.30 μm methylene blue; (d) 3.81 μm methylene blue.

3. Particle bounce is a function of both particle diameter and jet velocity. When particles are bouncing the collection efficiency is not a function of Stokes diameter, but also depends upon the jet velocity. Therefore, if bounce is to be minimized, it is preferable to use multiple jet impactors, which have lower jet velocity for a given cut size.

4. The performance of impactors changes as the collected particulate material piles up on the collection plate. The stage cut-off diameters decrease as the stage loading increases.

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DISCUSSION

HARRIS: Essentially it says that if the particle is not caught where we want it to be caught, it's going to be on the filter if it's not somewhere else.

RAO: Right

SMITH: In that case, you would be a lot better off throwing away your backup filter catch.

RAO: Yes. However, the filter catch can give an estimate of the magnitude of bounce. If bounce is severe, we would be better off eliminating the entire run than to get data, which results in distorted size distributions.

MCCAIN: It doesn't mean much. Do you have enough information on the rate of buildup of efficiency change with buildup of particles on the stage to estimate how much the effective cutoff of the stage will change as you do some sampling in a reasonable situation?

RAO: The study concerning the effect of load on impactor performance was the result of a half day's work. It was done along with Dr. Alex Berner of University of Vienna, Austria, who first reported the capillary action of the precipitate. Although the present data are limited, they clearly show another nonideal characteristic of the impactor, which needs some study.

ENSOR: Mercer reported a number of years ago that efficiency increases as you get buildup because you reduce plate-to-collector distance. Is the increase in efficiency due to decreased jet-to-plate distance with buildup?

RAO: Yes, the efficiency increases with decrease in jet-to-plate distance. However, the impactor chosen here has a jet-to-plate distance of 3.44 jet diameters. This spacing is large compared to the buildup on the plate, and therefore, decreased jet-to-plate distance and should not have contributed significantly to the observed increase in collection.

HARRIS: One problem we have to worry about is that the capillary action does not stop at the top layer--it just keeps on going into the air.

_____: How did you apply oil or grease to the Andersen?

RAO: By dissolving Dow Corning oil in toluene and spreading a few drops on the plate with a fingertip. The toluene is allowed to evaporate by placing the coated plates in a clean hood.

EXPERIENCES IN USING CASCADE IMPACTORS FOR TESTING ELECTROSTATIC PRECIPITATORS

Joseph D. McCain, Southern Research Institute

HARRIS: Now we'll ask Joe McCain of Southern Research Institute to talk to us about electrostatic precipitators.

MCCAIN: This presentation will primarily be a discussion of problems that we have commonly encountered in field sampling programs on ESP's on coal-fired power boilers and the significance some of the problems might have.

The first problem that we have is the location of the sampling ports (Figure 1). The typical sampling locations that are provided around precipitators, at least at locations where we have had to sample, very seldom are anything like 14 duct diameters downstream and 5 diameters upstream from flow disturbances. Generally the sampling locations are, instead, only one or two diameters from a disturbance. This means you have to sample an awful lot of points in order to get a decent traverse, if the velocity distribution is such that it is even possible to do so. Sampling a lot of points with impactors becomes very expensive. We generally compromise by reducing the number of points we sample in order to hold costs down. That automatically introduces errors into the data, particularly for large particles.

Figure 2 shows a typical size distribution taken in the field on a coal fired boiler, in terms of relative amounts of material in logarithmic size intervals. The particular ESP inlet data shown in Figure 2 are not typical of most installations on pulverized coal boilers. The distribution in Figure 2 is deficient in large particles because on this particular occasion the samples were taken downstream of a mechanical collector, which had removed the bulk of the large particles that would normally be present. The error bars shown are one standard deviation about the mean for a number of runs made on this particular boiler. In this instance, in order to conserve time, one impactor run was made at each of 16 inlet ports with a four-point traverse of the duct being made on each run. So Figure 2 shows 16 inlet runs

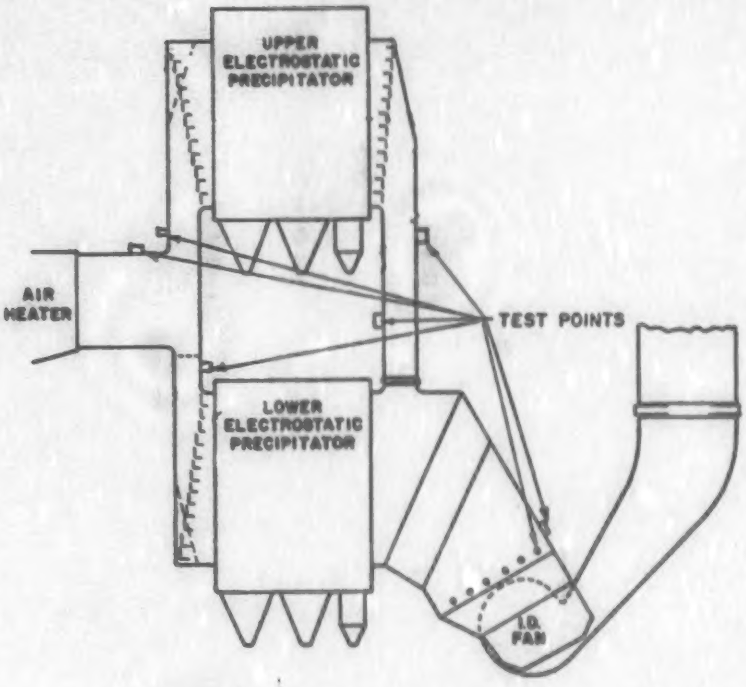

UPPER ELECTROSTATIC PRECIPITATOR
AIR HEATER
TEST POINTS
LOWER ELECTROSTATIC PRECIPITATOR
I.D. FAN

Figure 1. Typical locations of test points for control device evaluations.

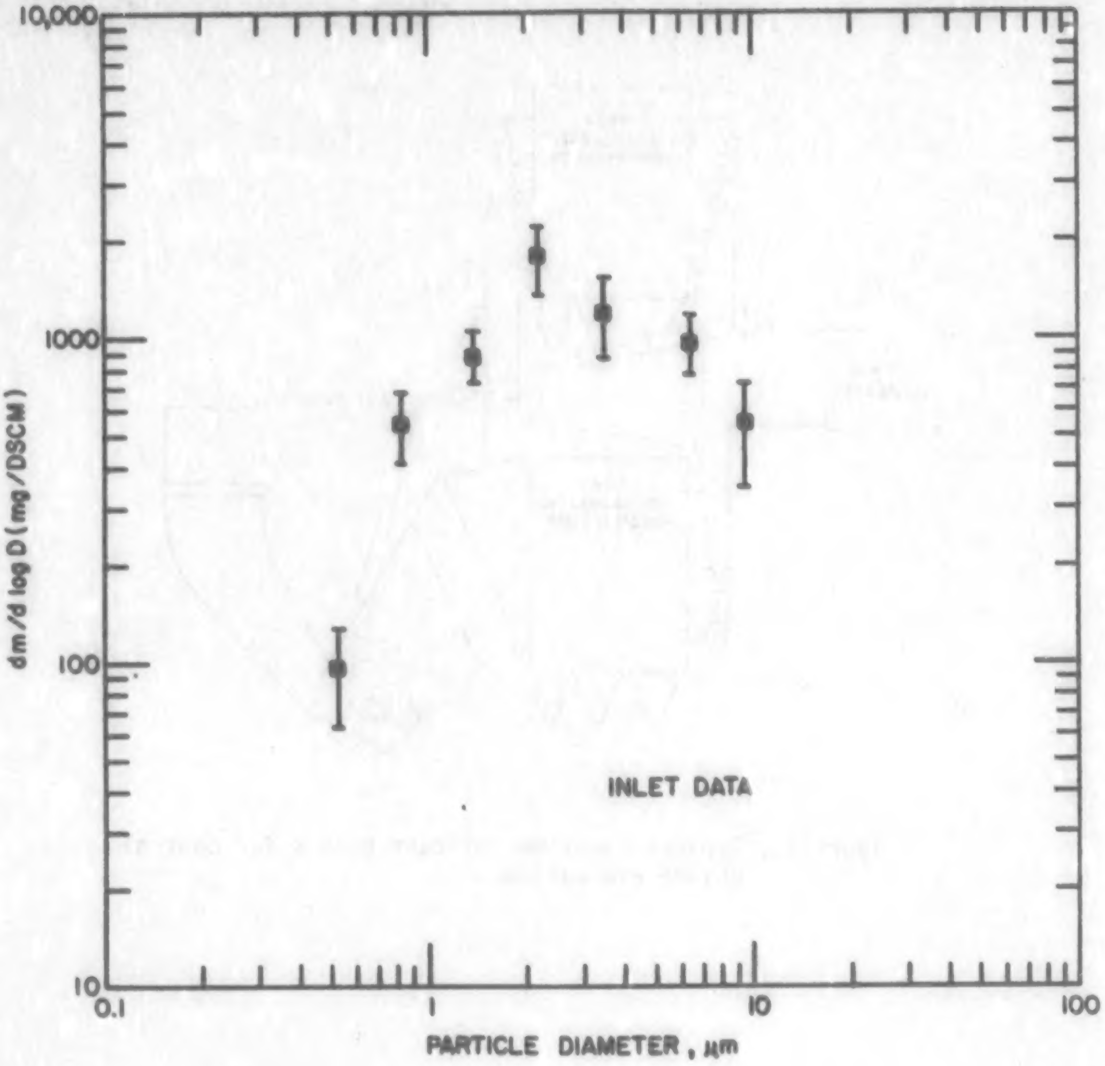


Figure 2. Average of the inlet impactor data for Naughton tests. The error bars show ± 1 standard deviation.

averaged. Part of the variation in the data is the result of measurement errors, and part of it may well be the result of variations in concentrations across the 16-port traverse. Mass train measurements, made at the same time that the data shown in Figure 2 were being taken, showed something like a 20- to 30-percent standard deviation in the total particulate loading, and that would be reflected in the impactors. Perhaps the size distribution changes that are occurring might be expected to show somewhat greater changes than encountered in mass train measurements.

Figure 3 shows outlet data, at the same location, with standard deviation bars. For fine particles, the scatter is relatively small. For larger particles, it becomes very large, approaching nearly 100 percent at 10 microns. There are several reasons for this; we'll go over a couple of them in a moment. There are two sets of error bars here, the smaller set represents another phenomenon which we will come back to later.

Figure 4 illustrates a major problem in measurements at the exit of precipitators collecting dry particulate. The precipitator plates are cleaned periodically by rapping. This leads to periodic changes in the size distribution with large swings in particulate concentration, especially in the large sizes. Figure 4 is a real-time particle monitor trace over a 48-minute period during a 1-hour rapping cycle on this particular precipitator. Each of the spikes is a rapping puff. They are quite evident in the 0.6 to 1.8 micron size range and somewhat less noticeable in the 1.8-3.5 μm range although they're still evident. It's not particularly evident that they're there in the larger sizes; however, they really are there as well.

Figure 5 shows the result of 10-minute integrations of the particle counter output over a 10-hour period. It shows concentrations of particles as measured with real-time monitors in 6- to 12- and 12- to 24-micron size ranges and shows one of the reasons for an extreme amount of scatter in the data obtained with impactors at the exit of precipitators.

About once an hour a major peak in concentration resulting from the rapping of the last field of the precipitator was expected. And in fact, we see that roughly once an hour we get such a peak. The magnitude of that peak is quite variable. (This sample monitor was operating at a fixed location.) About 10 o'clock we got a very large concentration spike, and again at about 3:30 we got a tremendous peak.

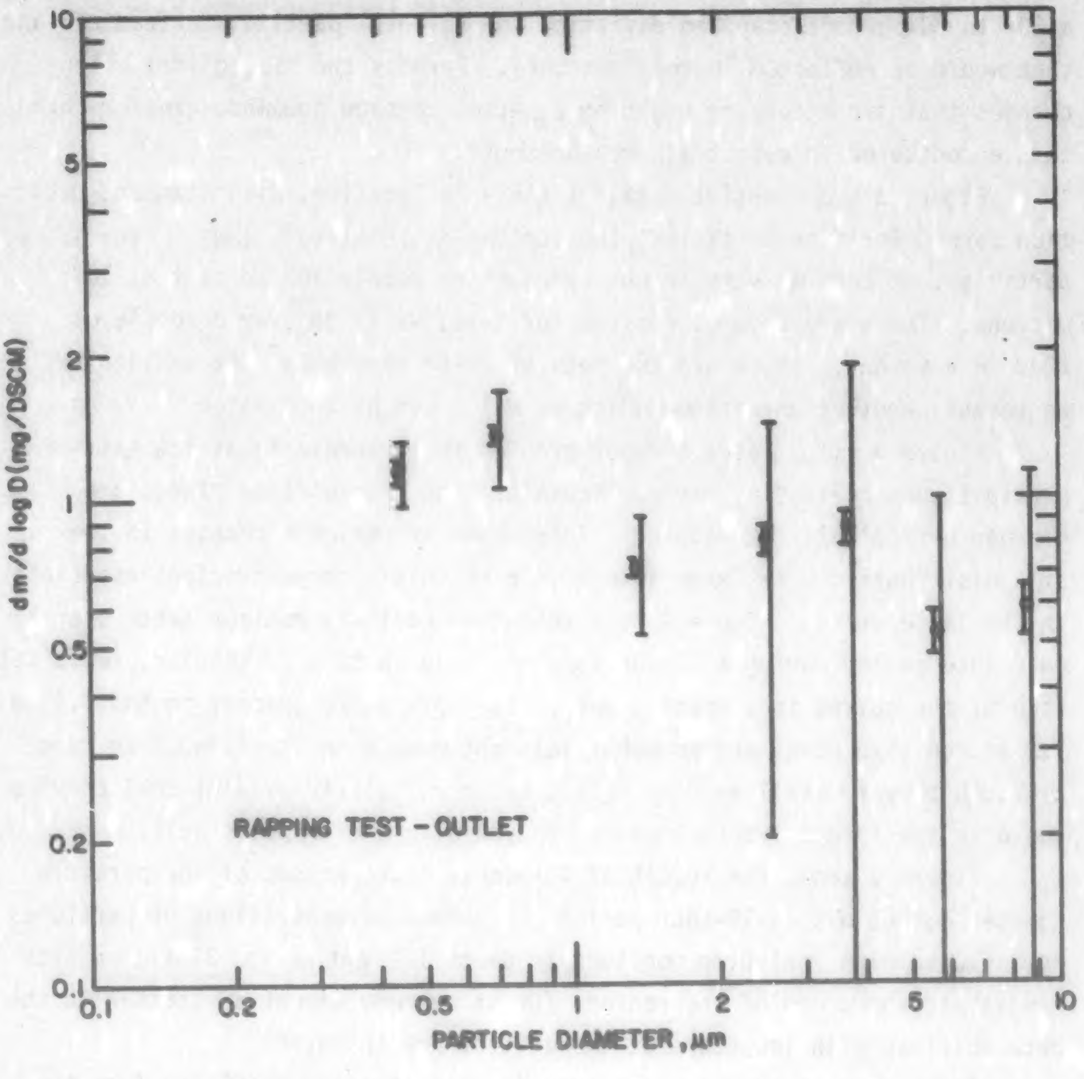


Figure 3. Average of the outlet size distributions. The larger error bars show ± 1 standard deviation of the particulate catches. The shorter error bars show the fraction of the uncertainty which can be attributed to the scatter in the blank weight gains.

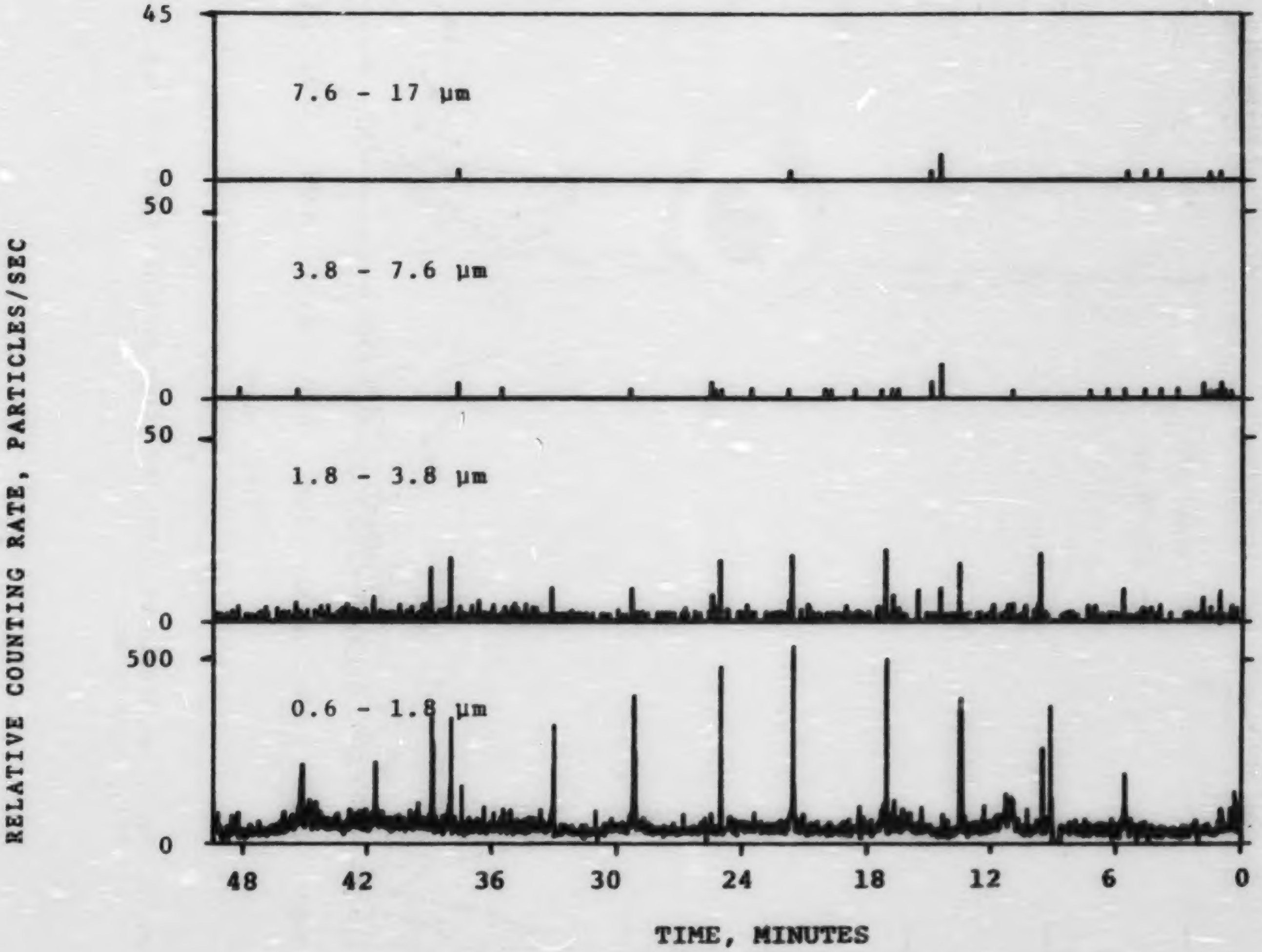


Figure 4. Typical data segment from large particle system, Aug. 6, 1975.

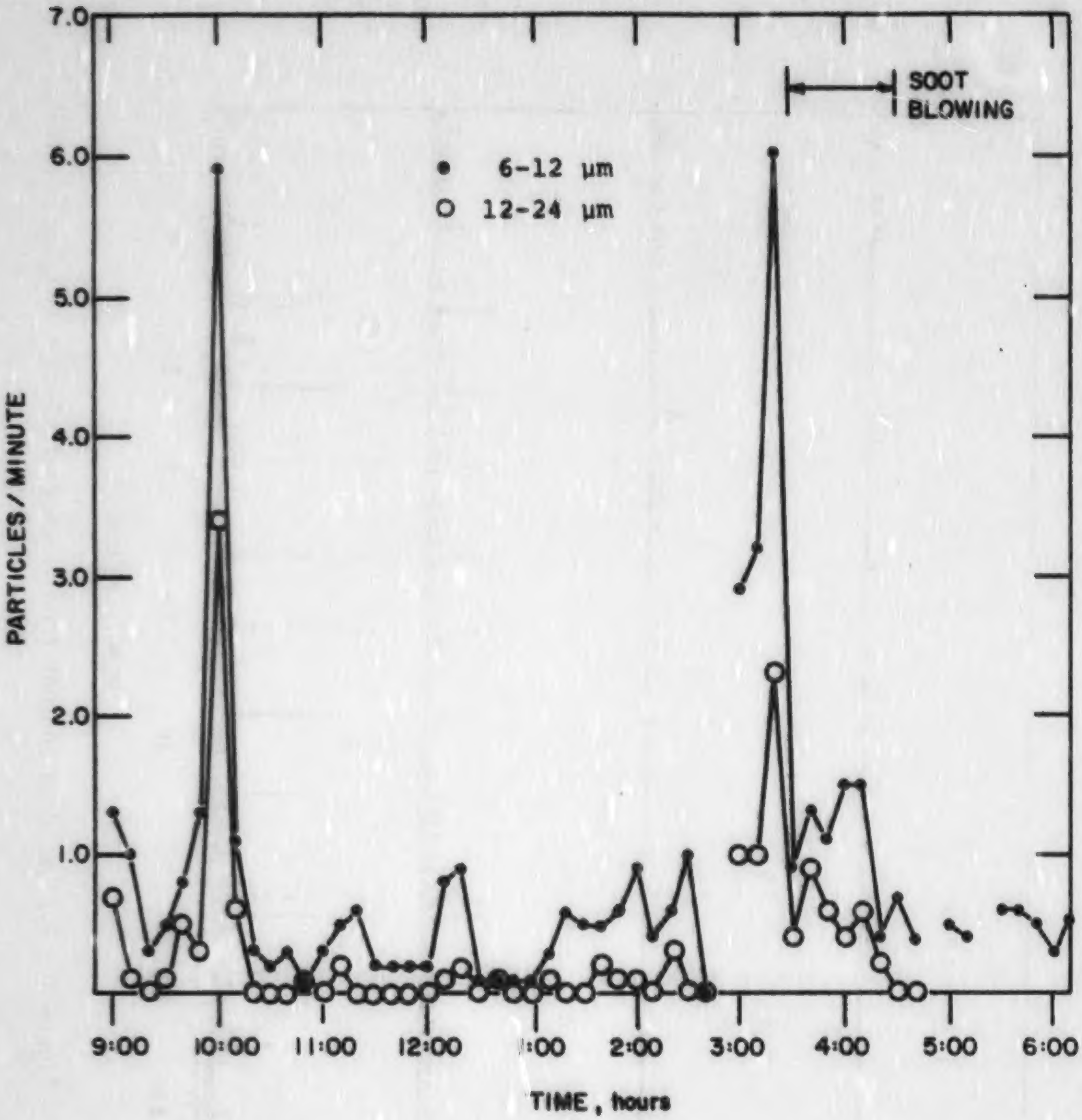


Figure 5. Particles/minute vs. time for large particle system on Aug. 5, 1975, rappers on.

Cascade impactors was sampling at the same time to provide mass data as time-integrated averages. These were the data that were shown in Figure 3(an average of three simultaneous impactor runs). The outlet loading at this particular precipitator, a 99.95-percent efficiency percipitator, was so low that the impactor integration times required to obtain reasonably weighable quantities were about 20 hours with the impactor flow rates which were selected. Each of the three impactors performed a 24-point traverse of the exit plane twice during the 20-hour sampling period.

The magnitude of these rapping puffs is spatially dependent. A large part of the rapping losses tend to be confined to lower parts of the ducts and as a consequence, the results that one obtains with the impactor depends on the phasing of the sampling locations with the rapping of the precipitator plates. A significant amount of scatter could be introduced, particularly in large particle sizes as a result of that effect. It gives one a tremendous headache in trying to analyze the data and determine what the efficiency of a precipitator is for large particles. One calculates that it ought to be very, very high. Figure 6 shows relative percentages of the total exit emissions resulting from the puffs for various particle sizes. At about 20 microns and up essentially everything appears to be in the rapping puffs. At 2 or 3 microns, most of the emissions appear to be material which has gone through uncollected rather than being collected material reentrained in the gas stream when the plates are rapped.

That material, which was rapped off the plates and introduced into the gas stream in the puffs at this particular installation, had a size distribution as shown in Figure 7. It was approximately log normal, with a mean size of about 15 or 20 μm. With a mechanical collector preceding this precipitator, there shouldn't have been much at 15 to 20 μm going in. The material in the rapping puffs appears to be agglomerates, which leads to another problem in interpreting the data. At the inlet to the precipitators we had primary particles--for the most part individual spherical particles. At the outlet the larger particles were agglomerates comprised of much smaller particles.

Stage one of the Andersen impactor, which we were using in this case for the outlet measurements, collected particles having diameters greater

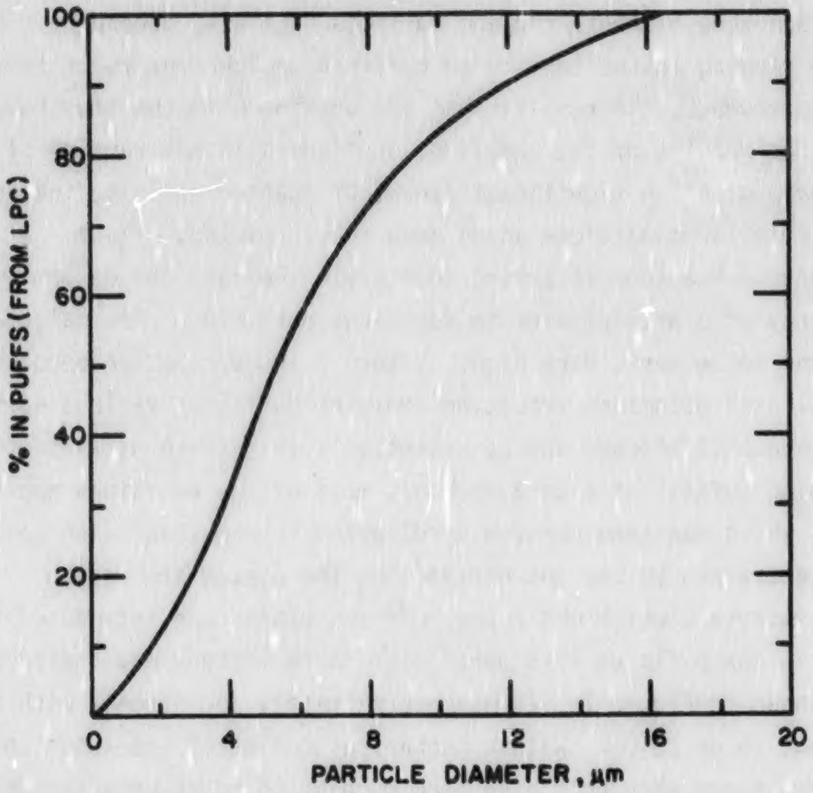


Figure 6. Contributions of rapping puffs by different particle diameters.

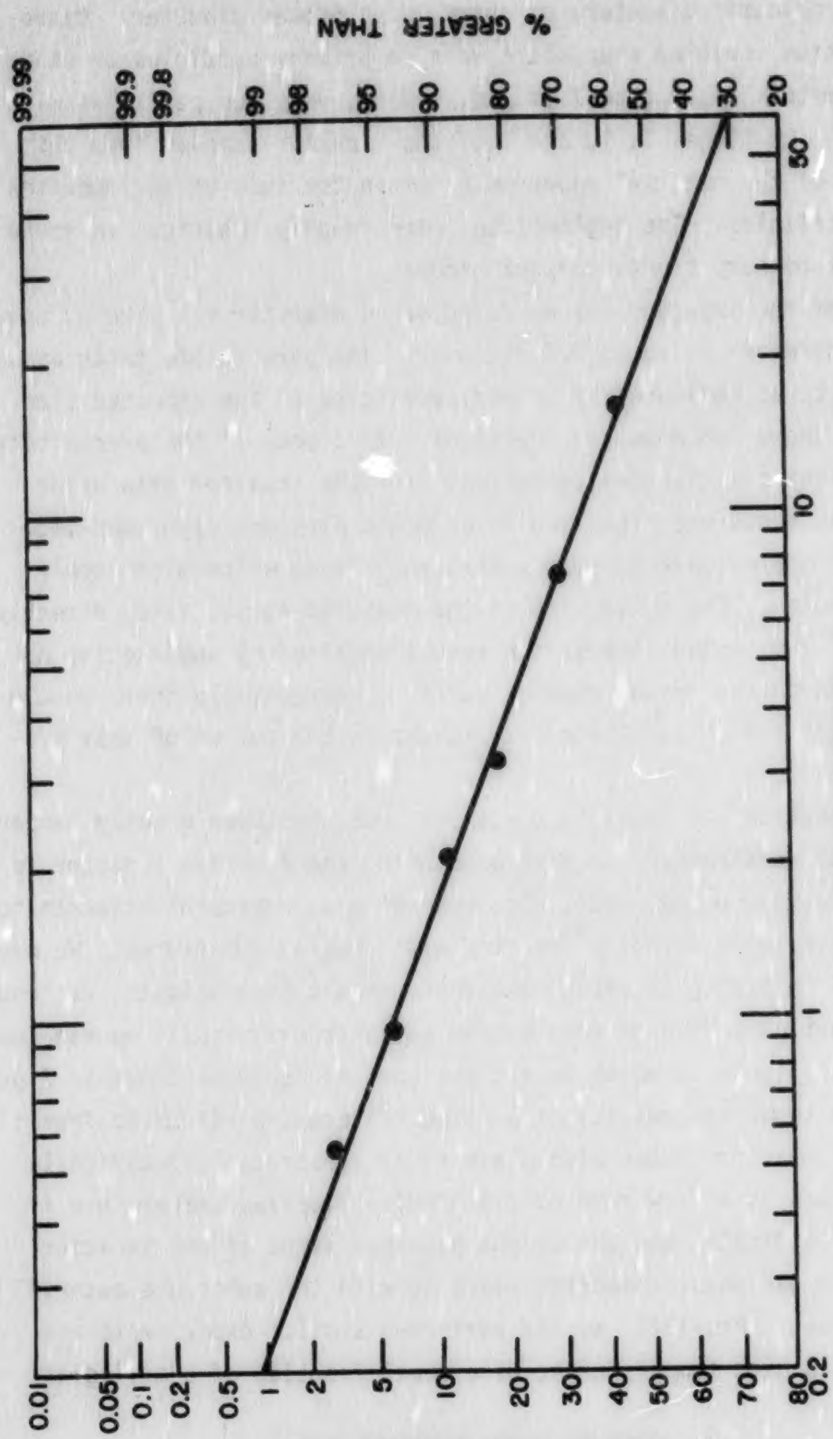


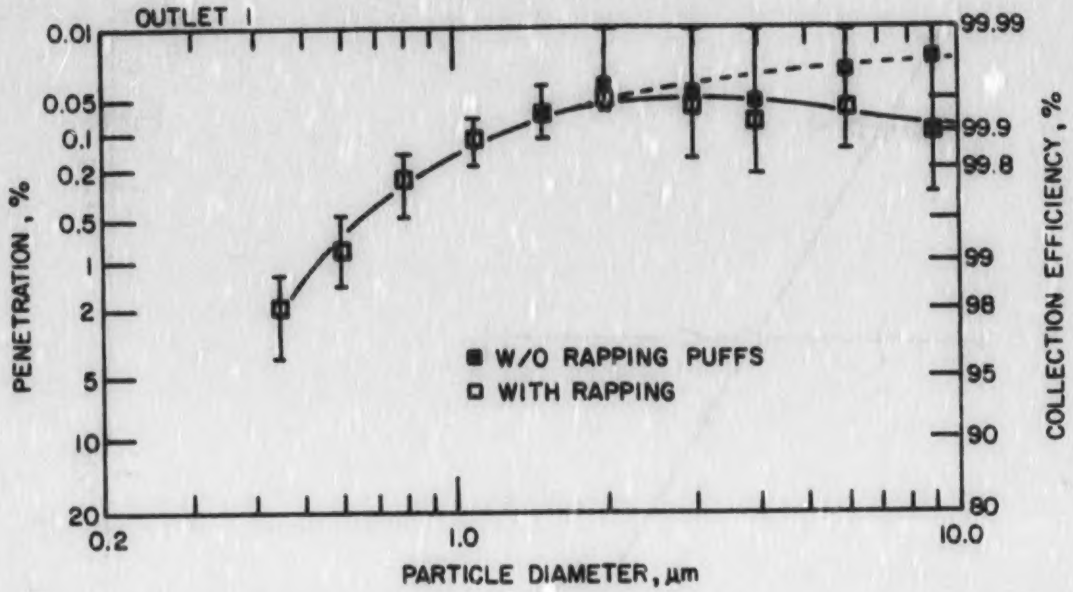
Figure 7. Cumulative mass distribution of a rapping puff.

than 16 μm (aerodynamic diameter) or about 10 μm Stokes diameter. Microscopic examination revealed that there were no primary particles on stage one with a diameter larger than 7 μm and, indeed, very few particles with 7 μm diameters. On Stages 1, 2, and 3 of the impactor samples from this test, the bulk of the material appeared to be in the form of agglomerates of 1 or 2 μm particles. The agglomerates were roughly spherical in shape and in the form of very tight, compact groups.

Stage 4 of the impactor has an aerodynamic diameter cut size of about 6 μm (Stokes' diameter of about 3.5 microns). The particulate catch of that stage appeared to be dominated by primary particles of the expected size.

Figure 8 shows the measured fractional efficiency of the precipitator. The bars represent efficiencies calculated from the impactor data using the outlet means minus one sigma and inlet means plus one sigma and vice-versa to get an approximate maximum uncertainty range which might apply to the efficiencies. The solid line is the measured curve, taken directly from the data. The dashed line is the result obtained by subtracting out the material introduced by the rapping puffs. Theoretically there should be nothing larger than 5 to 10 microns present in the outlet of that precipitator.

Data collection, at least in our experience, has been greatly hampered by problems with substrates. We have been using the Andersen impactor for the most part for our outlet data, although we've made several attempts to use the University of Washington impactor with greased substrates. We have been frustrated in trying to get grease that doesn't lose weight. We thought we had found that glass fibers that worked satisfactorily until we ran some tests at several plants at which we got the sort of behavior shown in Figure 9. This figure shows the results of pulling filtered, particulate-free flue gas through an impactor loaded with glass fiber substrates (specifically Andersen impactors at a flow rate of 0.5 ACFM). Flue gas temperature is shown along the ordinate, and the weight gain per stage of the impactor as a result of a gas phase component reacting with the substrate material along the abscissa. Pre-1974, we had performed similar experiments and gotten results at high temperatures, of a small fraction of a milligram



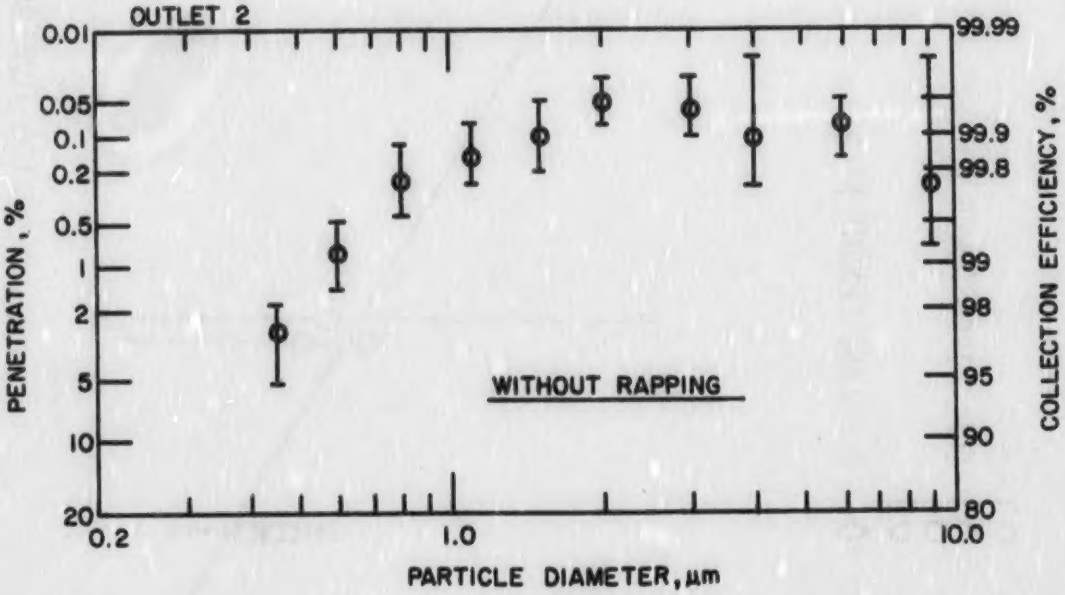


Figure 8. Fractional efficiencies for rapping and non-rapping tests.

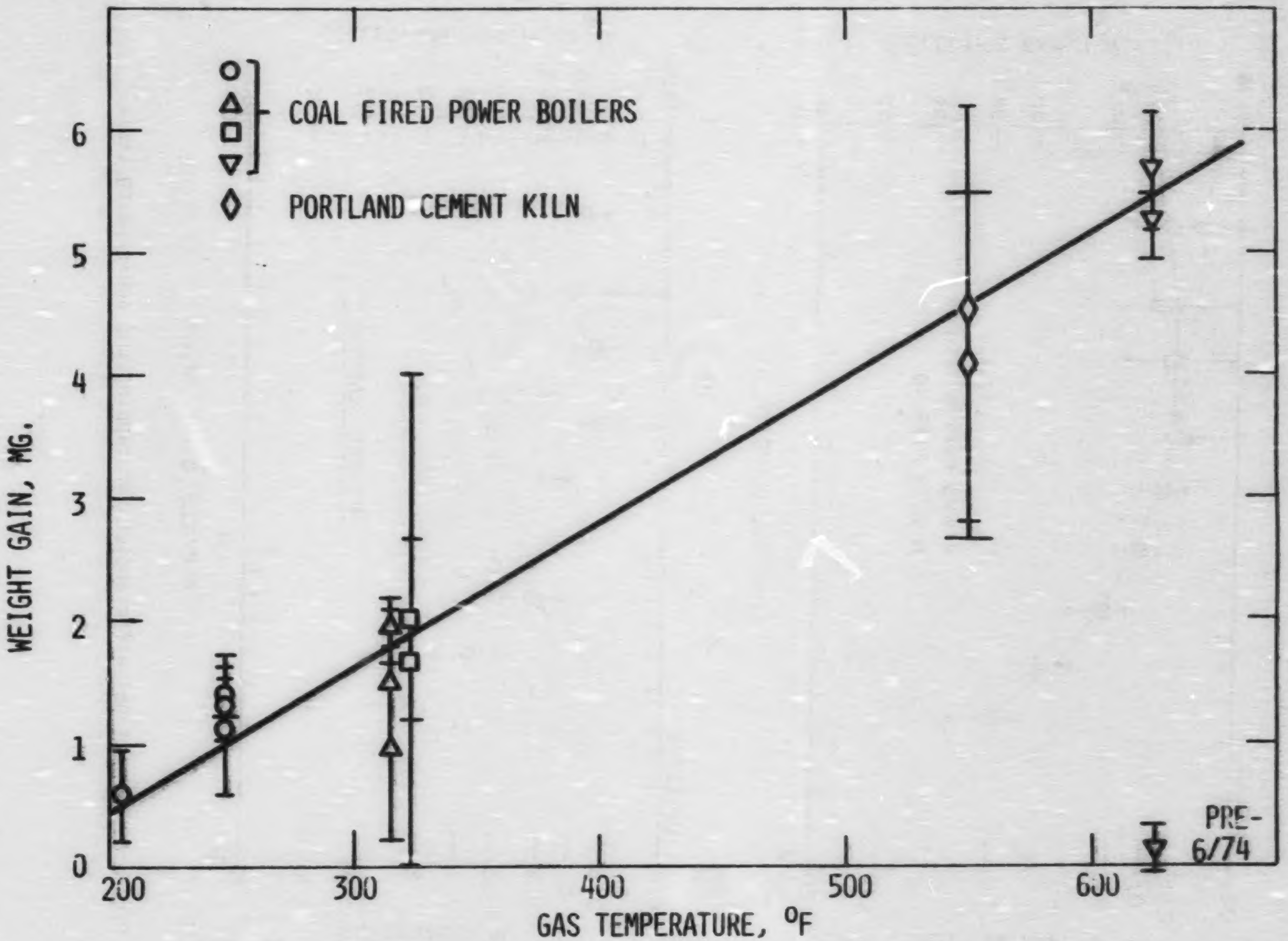


Figure 9. Anomalous weight increases of Andersen glass fiber impaction substrates at different flue gas temperatures.

weight increase from such a reaction. The filter manufacturers about June of '74 changed the process by which they make glass fiber filters. Whereupon we immediately started having difficulties. This same difficulty that we are having with the Andersen substrates in this case would be present in all glass fiber backup filters and Method 5 filters. Backup filters will tend to have problems in impactors anyway from material which bounces, resulting in oversize material arriving at the backup filter and contaminating it. In one case in a study done by Lawrence Livermore Labs, 98 percent of the weight on the backup filter was the result of oversize material and only 2 percent of the weight was the result of material that should have been there. This was with an impactor run with Nucle-pure polycarbonate material as impactor substrates.

ENSOR: Was that a stack sample?

MCCAIN: Yes, a stack sample at a coal-fired boiler. The backup filter was meaningless, in that case, for efficiency calculations. It had no size which you could really assign to it. If you get substrate reactions going with glass fiber backup filters as was shown in Figure 9, then you don't know what's happening either. Six milligrams is way in excess of the amount of material you might deposit on many stages and completely masks the catch. This effect is, we believe, a result of an SO_2 reaction with the substrate material. Once we found this to be taking place, we set off on a search for a substrate material--glass fiber--that would not exhibit such an effect. Figure shows the results for four of the materials that we tried. This is not an exhaustive list; we've tried a lot more. Teflon did real well; unfortunately, we found that it doesn't do very well as a substrate. Quartz (Pallflex quartz) fibers do very well, but they are so fragile that they are almost impossible to use as a substrate or a filter media for gravimetric work. Gelman has made a limited batch of an improved quartz material, which has about the same tensile strength as the standard glass fiber material. We have tried samples of that, and it is quite good for this application but it is not yet a commercially available product. A sample small batch was made primarily for pilot studies for EPA, and Gelman retained only a limited quantity for their own purposes. They may market it in the future. If they do, it will probably be a satisfactory backup material and

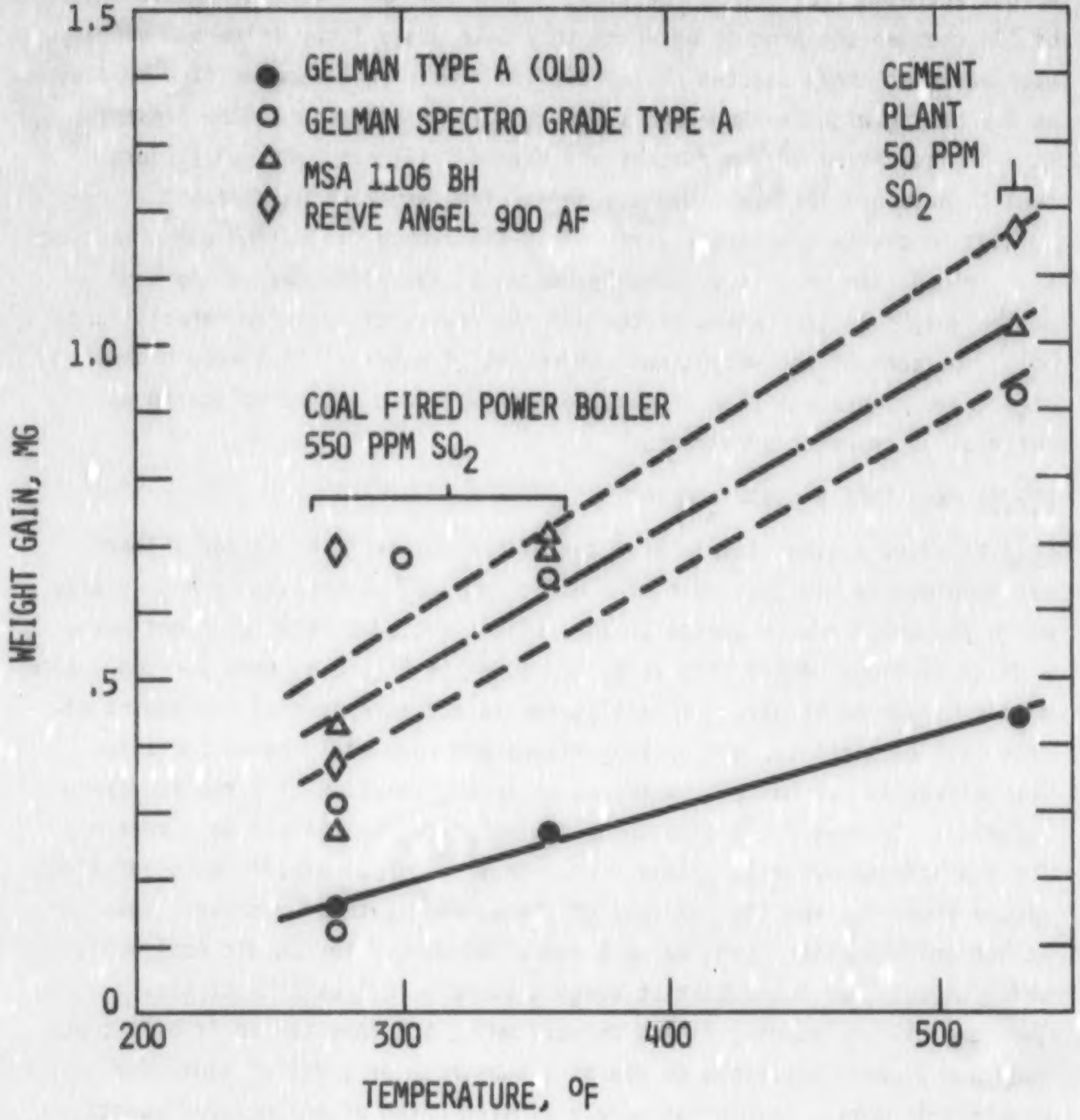


Figure 10. Anomalous weight gains of various 47 mm dia. glass fiber filters at different temperatures. (60 minute samples at flowrates of 0.25 ACFM).

substrate material. It will have to be tried in the lab as a substrate material to demonstrate that it does effectively reduce bounce problems. The materials shown in Figure 10 are all relatively unsatisfactory because of the reaction affect. The data were obtained with 47 -mm filters, somewhat smaller than the size used in most of the impactors that use glass fiber filters, although the Sierra does use 47 -mm. The weight changes are temperature sensitive for all of the materials; higher temperatures lead to greater uptake.

We have tried several experiments to alleviate this problem. One of them is to pull filtered flue gas through the material before we use it to precondition it. Figure 11 shows weight increases of Andersen substrates, once again resulting from pulling filtered flue gas through untreated material, resulting in weight increases of about 1.5 to 2 mg per stage. After running filtered flue gas through the material for a period of about 24 hours, then desiccating it, weighing it, using it as a substrate, the reaction affect was reduced to approximately one quarter of a milligram per stage, which is a much more acceptable level, although it still introduces some uncertainty in the data. That leads us back to the curve on the outlet data at the power plant previously shown. (See Figure 3).

The small error bars shown on Figure 3 are that part of the uncertainty or scatter in the data at this particular plant, which can be ascribed to the sulfate uptake (SO_2 reaction) of the glass fiber filter media substrates used in these runs. The large error bars are the 1σ limits of the data from the average of the three runs; the small ones are that part of it which would result from the SO_2 uptake. One can see that the problem that we have in this particular instance with the SO_2 reaction is very small with respect to the effect that it would have on the data. Figure 12 shows the correlation that was obtained by comparing gravimetric weight increases of substrates that had nothing but filtered flue gas pulled over them and sulfate determinations made by wet chemical techniques on those same substrates. Sulfate represents essentially all of the excess weight pickup that those substrates had. This particular data was obtained at temperatures

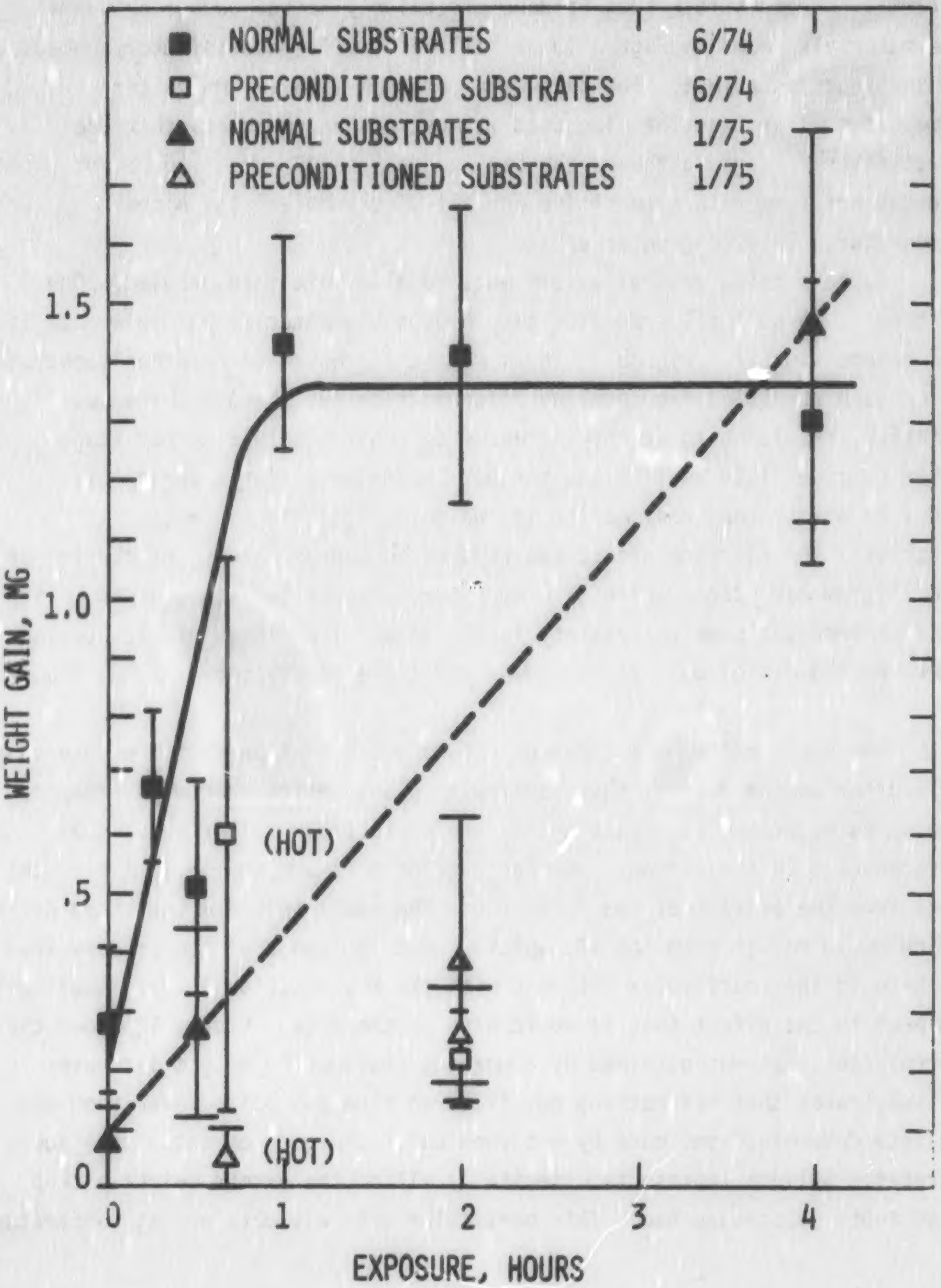


Figure 11. Anomalous weight gains of Andersen impactor glass fiber impaction substrates.

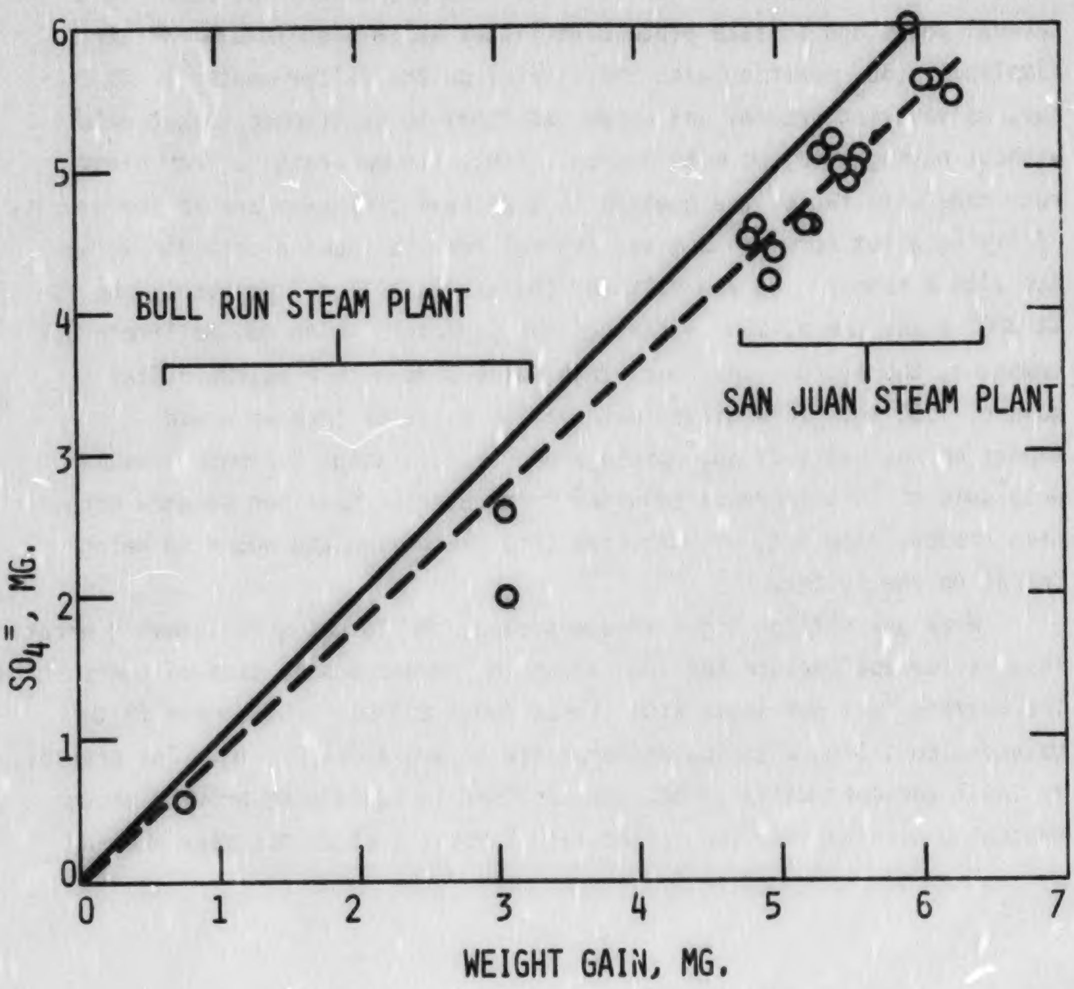


Figure 12. Comparison of weight of sulfate on blank Andersen Impactor substrates and observed anomalous weight increases.

of about 625° F at a plant burning a low-sulfur Western coal. It's hard to believe that sulfuric acid condensation would occur at those temperatures; on the other hand, SO_2 reactions with basic sites on the filter materials could very well account for this. This same phenomenon has been noted several years ago to take place with filter media used in ambient air sampling. (SO_2 reacting with basic sites on the filter media.[1]) As I say, we've tried greases and other impactors in an attempt to get data without having problems with bounce. Table 1 shows results from blank runs made with two of the greases that we have tried--we are in the process of trying a lot more--this gives typical results showing problems we've had with a couple. We ran filtered coal-fired boiler flue gases, one run at 300° F and one at 260° F through the impactor. With Dow Mollikote LLL compound, the average loss per stage was 0.5 mg with a backup filter gain of 1.86 mg. This filter material is material that we would expect on the basis of SO_2 uptake along to gain about 0.2 mg. Presumably some of this increase resulted from material that had somehow either been eroded, blow off, or outgasped from the grease and wound up being caught on the filter.

With Dow silicon high- vacuum grease, the losses were somewhat erratic. This is perhaps because the same amount of grease wasn't used on every stage. The average loss per stage with it was about 2.5 mg. The backup filter gained almost 3 mg when SO_2 was expected to add about 0.2 mg. The greases, at least the ones we've tried, when applied in quantities provide enough grease to wick up into the particulate layers, just do not stay around.

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[1] Forrest, J. and Newman, L., Sampling and Analysis of Atmospheric Sulfur Compounds for Isotope Ratio Studies, Atmospheric Environment, 7, pp. 561-573, 1973.

TABLE I

SUBSTRATE	DOW MOLYKOTE 111 COMPOUND	DOW SILICONE HIGH VACUUM GREASE
Temperature	300°F	280°F
Flowrate	0.46 ACFM	0.45 ACFM
Sample Duration	60 Min.	60 Min
STAGE	WEIGHT CHANGE (mg)	
1	-1.1	-4.06
2	-0.74	-1.74
3	-0.34	-3.60
4	-0.36	-3.76
5	-0.46	-1.32
6	-0.32	-1.90
7	-0.46	-0.64
Filter	+1.86	+2.68
AVERAGE LOSS PER STAGE	0.54 mg	2.43 mg
NET TOTAL LOSS	1.92 mg	14.34 mg

FILTERS - UNPRECONDITIONED GELMAN TYPE A (OLD TYPE)

EXPECTED FILTER WEIGHT CHANGE - ≈ 0.2 mg

DISCUSSION

ENSOR: Are those prebaked?

MCCAIN: These were prebaked; the material was put on the plates, baked in an oven for a period of several hours at 400° F (a temperature higher than they were actually operated at) and subsequently weighed and used. We have tried in the lab about 20-odd materials, about six of them looked good, two of the best-looking ones were these two.

ENSOR: How do you apply that grease?

MCCAIN: In this case, it was just put on the fingertip and smeared around right out of the tube.

_____: The important point is that your application is very, very important and hard to control.

MCCAIN: We have applied it at other times in the field in a suspension in benzene: Put on with a eyedropper and allowed to flow and then baked out at 400°. Again, we got negative weights at the end of actual runs when there was visible particulate on the stages. You could see that you ought to have a lot of weight gain but the results were negative weights. We tried several applications; these two were just smeared on with a fingertip. They weren't intended to be used as particulate runs, but just to see whether or not we were going to have a problem with losses, and we did.

_____: Do you think that it might be a function of time at all? In other words, if you baked them longer, you might see this.

MCCAIN: We tried this in the lab. We baked out, weighed, rebaked, reweighed, went through this treatment about four times. Several, including those in Table 1, tended to come to a stable weight in the lab. Out of the 20 we tried, about six tended to come to a stable weight. Those are the six that we were wanting to try in the field. As I say, these were two of those six, and their behavior with flue gases is somewhat different.

_____: You say these were baked more than 4 hours?

MCCAIN: Yes. At 400° F, a higher temperature than the application.

OLIN: This loss must have been a function of how much you put on; if you put less on, you get less off.

MCCAIN: Yes, yes, it does appear to be a function of how much you put on. Roughly 30 mg were applied to the stages in each of these cases. This is the University of Washington impactor; that's about what Mike recommends. At least, that's what the manual says.

OLIN: You put about the same on for each of the different greases?

MCCAIN: Yes.

OLIN: Same total weight?

MCCAIN: Same total weight. This (Molykote III) held up a good deal better than this (Dow high vacuum grease). Neither of them was particularly satisfactory. If you are collecting a few tenths of a milligram or even in this case a few milligrams of material, you would have difficulties in interpreting the results.

HARRIS: We've tried some GC types of greases that give conflicting data. Some of them that Joe's run that we had no problem with, he's had some losses with. Intuitively, it would seem to be a good candidate to use because the GC materials are usually defined at a certain temperature rating, and that temperature rating is something like the point at which one-tenth of one percent of their mass, so that they don't mess up the GC. So that would be intuitively a good source for it, but so far we haven't got data that tend to agree on it. We have a few runs on polyethylene glycol under laboratory conditions that indicate it stabilizes very quickly and you have no losses at 400° F.

_____: Joe, what is the percent grease in benzene in this first one here?

MCCAIN: Probably on the order of 5 to 10 percent weight. No more than 10 percent, and it was not a solution--it was a suspension, an emulsion obtained with ultrasonic agitation.

CALVERT: Joe, when you show that curve on the continuous monitor, what were you using to get that data?

MCCAIN: A Royco particle counter with an extraction and conditioning system. It did not provide data on absolute concentration. It was qualitative; you could look at concentration variations with time. We have done a couple of tests; one specifically a semicontrolled experiment in which we ran that system and obtained what I think is pretty good quantitative agreement between the impactors and particle counter. But it was in a situation where we could predict what our transport losses were in the probe and predict the transfer characteristics of the conditioning system. In this particular instance, we weren't able to do that. The data are not adequate.

_____: What was the change in the filter in '74 and was it only with Gelman?

MCCAIN: I don't know whether it was. The Gelman material, Type A, specifically, pre-June '74, was made in a sulfuric acid solution. That was changed to a hydrochloric wash, and it all went to pot at that point. point. I don't know if all of the manufacturers changed at that time or if some were using hydrochloric prior to that. I do know that we've been on the telephone off and on for some period of time now trying to get information from every manufacturer of glass fiber filters we could find, and none of them are using sulfuric acid currently.

_____: One other point about your SO_2 data is that at that temperature, your sulfuric would be mainly in the form of SO_3, and it could go through the same type of reaction that the SO_2 does. So you can't say that all that is due to SO_2 conversion; it could well be collection of SO_3.

HARRIS: One thing you want to look at is the point that the highest reaction conversion that he has was at a very, very low SO_2 concentration, and so the SO_3 would be very little.

MCCAIN: In that case the SO_3 was indetectable, and we had 50 ppm SO_2. We didn't expect to have any problems, we thought the SO_X content was so low that we wouldn't expect any problems at all, but it was one of the worst cases we have ever seen.

_____: Why do you suppose? Have you tried exposing the filter paper to pure SO_2?

MCCAIN: That is one of the next things to be done in the laboratory. This has all been done as preliminary work to a field test in each case to find out whether or not we were going to be able to obtain data and what to do to obtain data in that case. We're setting up a conditioning chamber in the laboratory using SO_2.

SPARKS: I think that, for completeness, it should be pointed out that some of our experience, some GCA ran and some experiments Bruce [Harris] ran in-house, there were problems with filter weight loss on blank runs.

HARRIS: Structurally weaker.

SPARKS: Structurally weaker, so there has been an increase in structural integrity so we don't lose weight anymore. But we gain weight.

MCCAIN: Those losses were relatively small compared to these gains.

____: They were of the same order as GCA's anyway, the same order as your reduced weight.

MCCAIN: We never found any at all in excess of a tenth or two-tenths of a milligram.

HARRIS: I think that the thing that you might be able to generalize is that the weight loss, Type A thing, seems to be a function of handling. It was something that you had a little bit more control over. If you had somebody in your field crew that was the meticulous type, you could control it a little bit more; somehow we can't control all those little buggers running around in the stack. It's just another indication that we had a problem with the weight loss, which was one reason we asked them (filter manufacturers) to try and do something about the structural state of these, so they did it for us. They fixed us real good. But, you know, a lot of what we're discussing here is:"Has anybody got any great ideas of how to cure all these things?"

OUTLINE OF FIELD EXPERIENCE WITH CASCADE IMPACTORS

Dr. Seymour Calvert, Air Pollution Technology, Inc.

HARRIS: We'll now head out to the West Coast to get our next two speakers to tell us what the problems are out there. Fortunately, they have to be the same problems that we have on the East Coast. First, Seymour Calvert will tell us all the woes that you get into at the tail end of scrubbers. Among other things, they're wet.

CALVERT: I hope that there will be plenty of time for questions, so what I am going to do is essentially outline the area and talk about troubles.

Just to sketch in the background from which I am going to speak, the experience we have had has mainly been concerned with scrubbers and fine particles. This means that we have gotten into inlet conditions that range from hot and dry gas to saturated and wet gas, and when I say wet here, what I mean is gas containing entrained drops of liquid mist. The inlet gas conditions are variable; it depends on whether the gas comes right from the source and goes in the scrubber, or whether it goes through some type of quencher or precleaner first. The outlets are generally saturated and generally wet, which implies that the entrainment separator isn't working very well. Sometimes we have to sample after a reheater so that, even though what came out of the scrubber was saturated and wet, what comes out of the reheater and what we sample is below saturation. The types of cascade impactors we've used are the Andersen, the Brink, and the APT--that's Air Pollution Technology--built impactor and the UW (Pilat) Mark III impactor.

Generally, the systems and procedures we've used are in accordance with the EPA draft guidelines for cascade impactor use. The sampling trains are comparable to the EPA trains, give or take a few parts. We've used some special trains and procedures that I will allude to a little bit later on. Sometimes we've had to combine cascade impactor measurements with EPA Method 5 measurements. This becomes important where the purpose of the task involves determining whether the source in in compliance with emission standards or not, and generally we have to take simultaneous samples at various points in the system.

The major points to consider really have been touched on by other people and definitely are covered in the draft guidelines that Bruce Harris has sent out, so I will skip very quickly through those. They include choice of the impactor components, substrate coatings, filters, methods of use, sampling points, procedures, data acquisition methods, measuring gas velocities, temperatures, pressures, what kind of substrates and sample treatment, what kind of weighing? I guess we're going to hear more about that. How to handle impinger and drier (gas drier) catches, what kind of data and forms to use, what kind of field laboratory and other work facilities do you need, where to do the data reduction, what to do in the field, what to do at home, and what kind of process data should you acquire. These things are more or less general and I'm not going to spend much time on them.

As to problems, have we got problems! The first one relates to sampling position, and I call this one, in my notes, "Picking the best of poor alternative sampling positions." This has been touched on before. You never get a good sampling position. There are problems with particle bias, with getting a good gas flow determination, and there are variations in gas velocity. There are problems because of the probe size and shape, which is required in order to cope with the geometry of the situation. There are problems with the cascade impactor orientation, which is necessary because of where the duct is, where the sampling points are, where the platform or nonplatform is located.

Coping with Process Variability

"How simultaneous can you get or do you have to get?" This has been touched on. Joe McCain talked about the problems of matching inlet and outlet sampling times. Generally, we're concerned with the determination of the efficiency for highly efficient devices which means that the outlet loading is going to be low, so that you have to sample for a lot longer time on the outlet or at a lot higher sampling rate, or in general you need a bigger sample volume on the outlet than on the inlet. Another problem in process variability is: "Can you replicate operating conditions?" Again, Joe touched on that with regards to such things as rapping in the precipitator. There is also the problem of process variability where the source has its ups and downs.

Optimizaing the Test Cost

Here, the question is: How much sampling apparatus do you need to get a fast turn around time? What crew size do you need? The problem is that you have to contend with plant shutdowns. The plant will shut down. I don't think we've ever been on one that there wasn't a breakdown. I used to be cheap (frugal). We tried to do tests with maybe two people, thinking: "Okay, we'll spend longer out there, but this will really be economical." You're asking for it. If you do that, you're sure to run into a plant shutdown, you'll have to bring your people back all the way back from Provo, Utah, or Casper, Wyoming, or someplace. Sure as heck the next time you go back there, there will be a blizzard. You have to face the cost of all that thermal underwear. So, we came to the conclusion that it was cheaper to use a bigger crew, buy more equipment, get in and out fast and keep down the possibility of being shutdown. Weather can also do it to you. So there are acts of man, and acts of God.

Transportation for Apparatus

When you have to go a long way from home, there is a question of how do you get the apparatus there: Should you drive, truck it, fly it, do without it? Then you run into an optimization problem. There is one of: Is it better to have a van for the purpose of hauling equipment out, or do you want to have a mobile laboratory where you can actually work in the van? Our decision has been to just use the van for transportation and use laboratory facilities, or get laboratory facilities onsite, some way.

Reliability of Apparatus

How much redundacy is good? After several years of hanging by our fingernails with meager data because meters failed, we now use three flow meters in series. Maybe its enough. We use a rotameter, we use a dry gas meter, and we use an orfice meter in series.

We check them against one another. Usually that's alright. Two sets of temperature-measuring instruments, extra pressure-measuring instruments, manometers, magnet heater gauges, etc.

Sampling Pump Leaks

For everybody else, sampling pumps don't leak. Ours always leak. They may start out not leaking, but they sure wind up leaking, and again the redundant meters on both sides of the sampling pump are a big help in detecting that kind of problem.

Heater System Burn-outs

We generally have to heat the impactor or the probe of the sampling line between impactor and filter. I'll get into this more later. We've had lots of trouble with the heaters burning out. Lately we've been using blanket type heaters where the heating wires are impregnated in silicon rubber sheets, and these have been pretty good. We're out of most of our troubles there, but they do burn out; variacs burn out and so on.

Corrosion

We have run into most of our corrosion problems when sampling gases which contain fluorides. If you want to get that sinking feeling, take apart your expensive UW impactor and see the pits drilled into the substrates and through the foils and then to the impaction plates. It might make Mike Pilat happy, but definitely not me. It even happens to our APT impactors, and so we have two impactors that we have machined out of Teflon for just such festive occasions. Incinerator gases are where we get into trouble. People are burning PVC wire, or occasionally they run into a little Teflon wire which gets hydrofluoric acid in addition to hydrochloric acid into the gas, and these give you trouble. Stainless steel is just no good for those.

Low Pressure Ducts

We've sampled downstream from a scrubber with an induced draft fan at -110 in. water column, and we lost one pitot tube and they wouldn't tell me what else had been sucked into that duct. It was terrible, and then there's the question of whether the sampling pump can cope with it and whether your apparatus can cope with it. Is everything build so it will work properly with that kind of high negative pressure, vacuum?

Large Ducts

Here we're generally talking about power plant sampling situations where the duct might be 15 or 20 or 30 ft across. I know that the Southern Research people have run into this very often. There are problems with just handling the equipment; probes that extend across that length or up and down that length are hard to handle. Just lifting the apparatus and then being able to determine the position of impactor or the pitot tube or whatever it is dangling at the end of a 16 ft long pipe is difficult. If the gas velocity is fairly high and there is some flexibility to the apparatus, the question is, Where is it; where is the end of it?

Crew Pollution

We were prepared for ammonia because we sampled urea filling towers, and we were prepared for SO_2 and, son-of-a-gun, when we went up to a large foundry, the guys came down with carbon monoxide, i.e., with headaches. They ran into very high CO exposure, so we had to invest in masks for that too. High temperatures can be a problem; obviously particles also--the respirable range and even the gritty type particles. Entrainment can be a problem. If you have ever had the experience of sampling downstream of a scrubber on a coal drier, it's a big thrill to be coated with the black mud that comes spewing out of the exhaust. Noise can be a problem, too.

Entrainment

I have talked some about entrainment that can get onto the people. The entrainment inside the duct is also a big problem. We have developed a precutter, which is essentially a one-plate one-stage impactor that has enough volume to handle most situations. However, we've run into situations where it was just swamped. There was so much water coming through that we couldn't handle it with our precutter and we couldn't handle it with a little cyclone. We invented what is known as the "Beer can entrainment separator." I can discuss it with you over a cold Coors or something. It was fashioned on a western sampling trip out of a you-know-what and essentially it amounts to using a big baffle. Here we get into compromises which I'll talk about later. The question is: can you live with that kind of sample bias?

Particle Size Changes

Actually, that's what I'm going to talk about mostly. The particle size is not constant. We have a friend who is a world traveler and he can gain or lose 50 lbs. He would go on a big trip around the world and gain 50 lbs and go on a crash diet and lose it and he had suits for various stage weights. You never knew exactly what size the guy was until you saw him; and particles are like that. Water condenses on them or evaporates dependent on temperature, pressure, and partial pressure on whatever this volatile material is.

Correlation of Cascade Impactors with EPA Method 5 Samples

Now we have to be concerned with sample bias with losses in the impactor and what we call "mystery particles" which are caught in the impinger. We've run into this case where something winds up in the impinger. We've had as high as 30 or 40 percent of the total particulate found in the impinger, an ordinary Greenburg-Smith type impinger following an UW impactor with the final filter. If you have to work with plant people or control agency people who are not as sophisticated as us guys, they will say;" Well, it's the fine particles that get through the filter." You try to tell them that the filter stops everything and they don't believe you. They make remarks about those long-haired research types and so forth, but this has so far proven to be attributable to previously unknown volatile materials that were present in the samples. So far we have been able to rationalize what has happened, but it has taken work.

Electrostatic Charge Effects

(1) Joe McCain mentioned that you can get electrocuted.

(2) The impactor measurements are affected. We've run impactors with and without radioactive charge neutralizers in a precutter type assembly. A preneutralizer at the impactor has an effect on the particle size distribution measurements.

Cleaning and Checking Cascade Impactor Jets

How do you know that they are clean? How do you clean them? We're going to have to hurry here because of the time.

Drying Samples of Volatile Material or Volatile Samples

Again, its a discouraging experience to find out that the guys were out in the field three days sampling a urea prilling tower scrubber, they put the samples in the oven, went through the usual procedure, and all the sample evaporated. Then you find out that urea is volatile and so is ammonium nitrate and other things that might be in there. It's all very interesting and educational, but it sure isn't profitable.

Coping with Particle Size Changes

I'll just run through this very quickly. One type of problem is due to the fact that you cannot take your choice of sampling position. The condition of the gases is variable. You have hot gas that may go through a quencher before it gets into the scrubber. It may then go through the scrubber, and it is wet when it comes out. If it does not go through a reheater, it will go to the fan and come out wet. There may be a reheater so it will be dry when it goes into the fan.

As Les Sparks mentioned in the beginning, the first big question is: "Why do you need the data; what are you trying to do?" If the purpose is to develop or validate a model for the scrubber, then you want to know what the scrubber saw. What size were the particles at the end of the inlet of the scrubber and at the outlet of the scrubber? If you don't have a sampling poing at these locations, and you have to get a sample at the point where the gas is dry, the question is how much did the particles grow? Did they grow because they nucleated condensation and they all grew to about the same size as in a cloud chamber (or condensation nuclei counter)? Or did they grow because they go into solution and lower the partial pressure of the water, so its an equilibrium type relationship?

We have used two approaches. One has been to run the impactors heated so that the particles are dried out at the inlet and at the outlet of the scrubber. Alternatively, we run the impactors unheated so that we can match inlet and outlet conditions. There was one case where we couldn't get the wet particle size because there wasn't a way to sample it. We took samples from the inlet through a humidifying flask, which was a very low efficiency impinger having a large diameter tube to bubble the gas through; took the sample through another flask simply to insure that it reached equilibrium; and then we split the sample and put it into one heated impactor and one unheated impactor. There was a difference between the wet particles; i.e., the particles in the unheated impactor as measured with the unheated impactor and in the particles measured by the heated impactor.

I think I'd better just stop here. I'll be pleased to answer questions whenever there is time.

DISCUSSION

____: What is the difference in that size distribution in the mean size as you go from wet to dry or vice versa?

CALVERT: This run was about 1.5 times, another one about 1.6. There were some other runs. Here's one at about 50 percent. You're going from about 1.3 to about 2 microns mass median diameter between the two runs.

____: What kind of dust is that?

CALVERT: This is borax. We've taken similar data on a composite dust which was diatomaceous earth, which contained sodium chloride. We found considerable particle growth with that. Apparently, there was more of a nucleation effect with that one. In other words, the particles tended to be fairly uniform in size, which would indicate a nucleation of condensation effect rather than just an equilibrium growth.

SMITH: What kind of source did you do your charge neutralizing test on? How large was the effect?

CALVERT: That was a titantian dioxide test dust, and it was on a charged drop type scrubber, and I can't remember how big the effect was. We've just gotten a draft report on it, and i'll check it for you later. There was a definite effect on running with and without a charge neutralizer just ahead of the cascade impactor. Also, while I mention that, we also used a charge neutralizer on the dust feeder and found a definite effect on the efficiency of the scrubber as measured with that test dust, due to electrostatic charger on the test aerosols.

BOLL: Your data on the borax implie to me a bimodal distribution, and yet you drew a straight line through it. I'd be curious as to why you did that.

CALVERT: We just characterized the dust with that simple distribution. I would say you are right and it is bimodal. It is a combination of a condensation fume and dust from a rotary kiln. There are definitely two sources; large dust and small dust. I think that we were not that certain of the lower cut points at that time, and I think this was prior to the time when we had calibrated our impactor. By the way, we too have calibrated our impactors, and we used polystyrene latex and a light scattering type counter. We have used that technique, and if we were doing it again I think that we might be more fastidious.

BOLL: The reason I asked is that this type of curve seems to be a rather typical occurrence and the straight line seems to be a rather typical reaction. Of course there is a big difference in what your collector is going to do depending upon what the truth is. Unless you have some reason to believe the cut sized wrong and the data are wrong and the straight line is right, for heaven sake we need to know.

CALVERT: Yes. We draw the straight lines; however, where we draw them depends upon what we want to do. If we were interested in collection efficiency on the small sizes, we would bias the line toward that end of the curve. Another problem in here is that (I guess we didn't use a precutter on this one) if we use a precutter you have the influence of that on the upper stages of the impactor. It's collecting the bigger particles and that's a superimposed effect.

BYERS: On the determination of the efficiency of your control equipment, obviously you're looking at particle size distributions and their impact on the determination of the efficiency. What would you consider a significant change in particle size distribution in terms of the mean diameter? What would have an impact on the accuracy of your efficiency determination? In other words, what is the kind of accuracy that you're looking for on efficiency determinations and what would be a significant size distribution change which would impact on that?

CALVERT: If you're talking about overall efficiency, I think Les Sparks gave the answer that 10 percent change in mass median diameter can have a doubling effect on the pressure drop requirement. We generally are not looking at overall efficiency nor overall penetration over the whole size distribution. We are looking at penetration as a function of particle size, so that we've been more concerned with "what is the effect?" than "what is the error?" and I am afraid that I cannot give you a nice clean answer to your question.

FIELD EXPERIENCE WITH CASCADE IMPACTORS: QUALITY CONTROL OF TEST RESULTS

David S. Ensor, Meteorology Research, Inc.*

HARRIS: Now I'll ask Dave Ensor to come up and describe some of his feats of magic and give us some of the results of the work. I guess it's basically some of the work at Nucla. Some is going to be on some substrate work on that thesis. He is one of the boys who has managed to go out to this place in Colorado about three miles past the edge of the earth, so he's a world traveler.

ENSOR: Thank you, Bruce. As Bruce mentioned, we were doing some work with the Electric Power Research Institute at Nucla, Colorado. The quality of particle size distribution data obtained with an in-stack cascade impactor depends on the magnitude of weighing errors and weight changes of the collection substrate due to handling and chemical reactions with reactive gases. Procedures using both blank impactor tests and control collection discs were developed. These procedures were used during a recent evaluation of a fabric filter on a utility boiler.

INTRODUCTION

Background

Cascade impactors are becoming widely used for the determination of particle size distributions by mass for control device evaluations. During the last year, it has become apparent that attention must be paid to chemical and physical changes of the collection surfaces. Fegley et al (1975) examined the sensitivity of the test results to the weighing errors and the impactor construction tolerances. It has become mandatory to check impactor results for weighing errors and undesired weight gains or losses.

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*Coauthored by Robert C. Carr, Electric Power Research Institute.

Scope of Paper

The scope of this paper includes:

- description of the MRI cascade impactor and substrates,
- report of the quality control efforts from a recent field test,
- indication of possible areas of improvement in testing.

MRI IMPACTOR SYSTEM

Introduction

In a particle size distribution measurement program, there are many important aspects of instrumentation:

- number and type of impactors,
- particle collection substrates,
- type of analytical balance,
- support equipment such as cleaning facilities and dessication chambers.

Description of Impactor

The MRI cascade impactor is an annular jet-collector type, similar to that reported by Cohen and Montan (1967). A cut-away drawing of the instrument is shown in Figure 1. The body of the device consists of quick connect rings supporting jet plates, collection discs, and a built-in holder. The design permits flexibility in application to various sampling situations. For example, the same impactor is used with three special jet stages to sample particulate matter with a light aircraft.

The impactor has been used with sampling rates from 0.15 to 2 cfm depending on the jet plates selected.

One very important aspect of the impactor is the collection disc. The disc is a lightweight metal stamping (730 mg) of 2-mil-thick 316 stainless steel. The disc is used only once, permitting a permanent record of the test. Also, this approach lends itself to the preparation of substrates for chemical analysis (Ensor et al., 1975).

The surface of the collection disc is prepared with a solution of high vacuum grease in toluene. The solution is painted onto the disc. It has been found that the thickness of the coating is important in the performance of the impactor. A common error is to apply too much grease. After air drying, the discs are heated at 400° F for 4 hours to remove volatiles. The collection discs are handled with clean forceps by the edge to prevent conformation and weight changes.

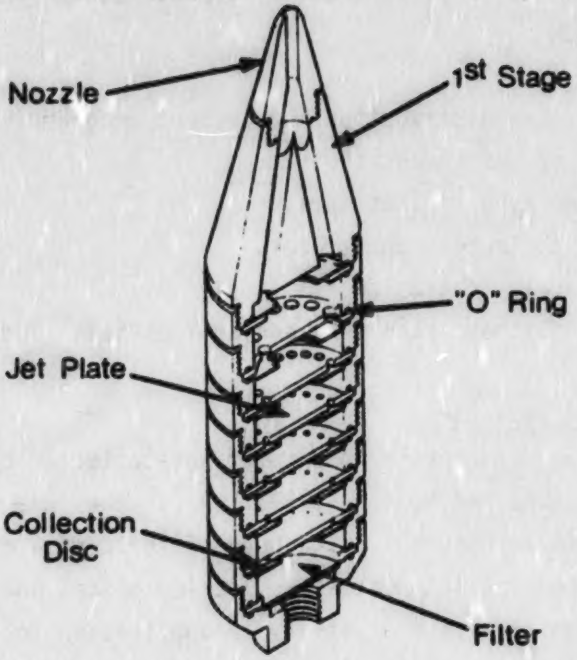
Nozzle
1st Stage
"O" Ring
Jet Plate
Collection Disc
Filter

Figure 1. Assembly drawing of Model 1502 Inertial Cascade Impactor.

The filter is held by Teflon contact surfaces and is backed by a porous metal plate. Teflon washers are used to prevent loss of the filter to the contact surfaces, and a tared aluminum foil disk is used to weigh the filter and Teflon washers.

Weighing

The weighing of the collection discs is a critical part of the whole test. The collection disc is designed to fit the weighing chamber of a Cahn 4100 Electrobalance. The Cahn balance offers the advantage of being portable (it is normally transported in a carrying case as hand luggage), and is relatively noncritical as to the stability of the support table. It is desirable to do the weighings at the motel to avoid disruptions due to low frequency plant vibration. Often a separate room is rented solely for the weighing. The discs and filters are desiccated for 24 hours before weighing to stabilize the water content.

One of the elements of success with any balance is to thoroughly know the balance and to develop optimum weighing procedures. A weighing by substitution method was developed.

TEST PROGRAM

Introduction

Impactor data quality control procedures were used in an evaluation of a fabric filter on a utility boiler in a project for the Electric Power Research Institute. The requirements for the quality control checks were motivated by:

- the reports of Smith et al (1975) of reactions of glass fiber filter mats with stack gas
- the stainless steel collection disc with a grease coating had not been thoroughly field tested
- the low outlet concentrations required long test times and light stage weight increases

Test Plan

All of the quality control tests were conducted with impactors used at the outlet of the control device. Two types of tests were used.

Controls -- Collection discs were prepared normally, transported to to the test site, but not mounted into an impactor. Since the outlet impactor was operated without the last jet stage, the collection disc normally used at the last stage became the control with little disruption of the test. The control collection disc was a good check of the performance in weighing of the samples. The tests of weighing repeatability must be conducted with the collection discs under field conditions during the normal pattern of work. Thus, problems with static charges, balance adjustments, and handling can be identified.

Blank Tests -- Blank tests were impactors prepared in the normal manner which sampled only filtered stack gas. These runs should identify problems from chemical reactions of the substrate with stack gas, loss of substrate from vaporization or abrasion, and contamination of the substrates from leaks, or during assembly and disassembly. The blank runs also had at least one control disc.

It was recognized that the physical and chemical nature of the greases are important in the performance of particle collection. A large number of greases and adhesive materials were screened in a laboratory study last spring; three materials showed promise:

1. Apiezon-L high vacuum grease: hydrocarbon with an average molecular weight of 1300,
2. Dow Corning 111 Compound Silicone Lubricant,
3. Hooker Fluorolube GR660 Fluorocarbon Lubricant.

RESULTS

Introduction

The actual test program was conducted in two two-week phases to allow the evaluation of the control device for various conditions of coal and to adjust the procedures between tests. All weighings were conducted in the field.

Controls

The Phase I results for the controls are shown in Figure 2. The standard deviation of the weighings are within the precision claimed by the manufacturer. The extreme deviations were isolated to only a few days of the test program. Since all the weighings were conducted by a single individual, it was suspected that perhaps the balance calibration

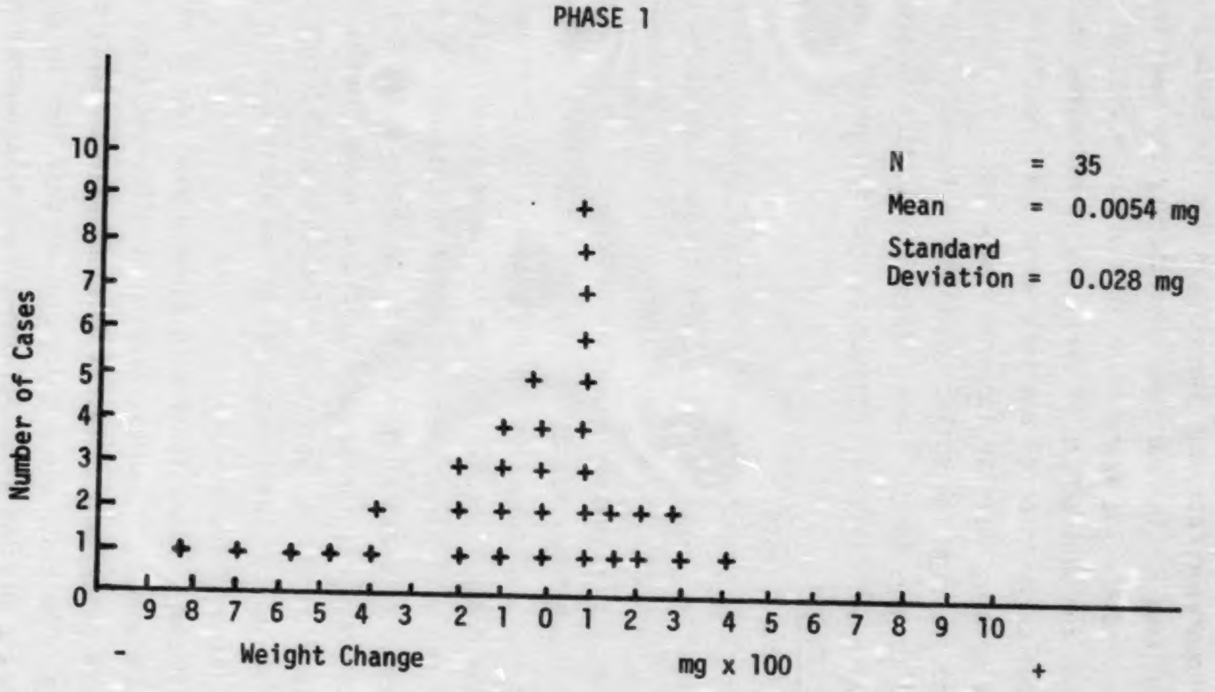

Figure 2. Weight change of control discs (gross weight 730 mg).

procedures required modification, that static charge effects were severe on some days, or the weighting tasks of the program should be modified to reduce operator fatigue. The results for the Phase II weighings are shown in Figure 3. The work was conducted by the same individual as the Phase I test. The improvement in standard deviation was thought to result from frequent checks of every couple of hours, instead of twice each day, of the calibration of the instrument and a reduction in the total number of working hours by improved scheduling. In future tests, it is planned to preweigh the collection discs in the laboratory and weigh only the final samples in the field. This modification in procedure will reduce the possibility of operator fatigue influencing the results.

Blank Tests

1. Phase I Tests

The results for the Phase I blanks are shown in Table I. The Dow Corning 111 Compound and the Fluorolube GR660 grease both had weight losses, while the Apiezon vacuum grease had weight increases. The random nature of the Apiezon weight increases led us to suspect that reactions with stack gas were not the primary reason for the changes. Examination of the plates revealed that contamination may have caused the increases in weight. During the Phase I tests, the impactors were scrubbed with acetone, followed with a rinse with clean acetone. After results were known from the first blank tests, more attention was paid to the impactor clean up. The reduction in weight change in run 56 is attributed to the greater care taken in clean-up. Work with the Dow Corning 111 Compound and Fluorolube grease was stopped because of the instability of the materials.

2. Phase II Tests

The Phase II blank tests were planned with the objective of testing the improved cleanup procedures, testing at different times to detect reactions with stack gases, and to test a Apiezon-Mylar preparation for trace element sampling. The trace element preparation described by Ensor et al (1975) involves the placing of an Apiezon-coated Mylar washer on the collection disc.

The cleanup procedure was modified by an ultrasonic clean in soapy water, a rinse in acetone, and a final rinse in clean acetone.

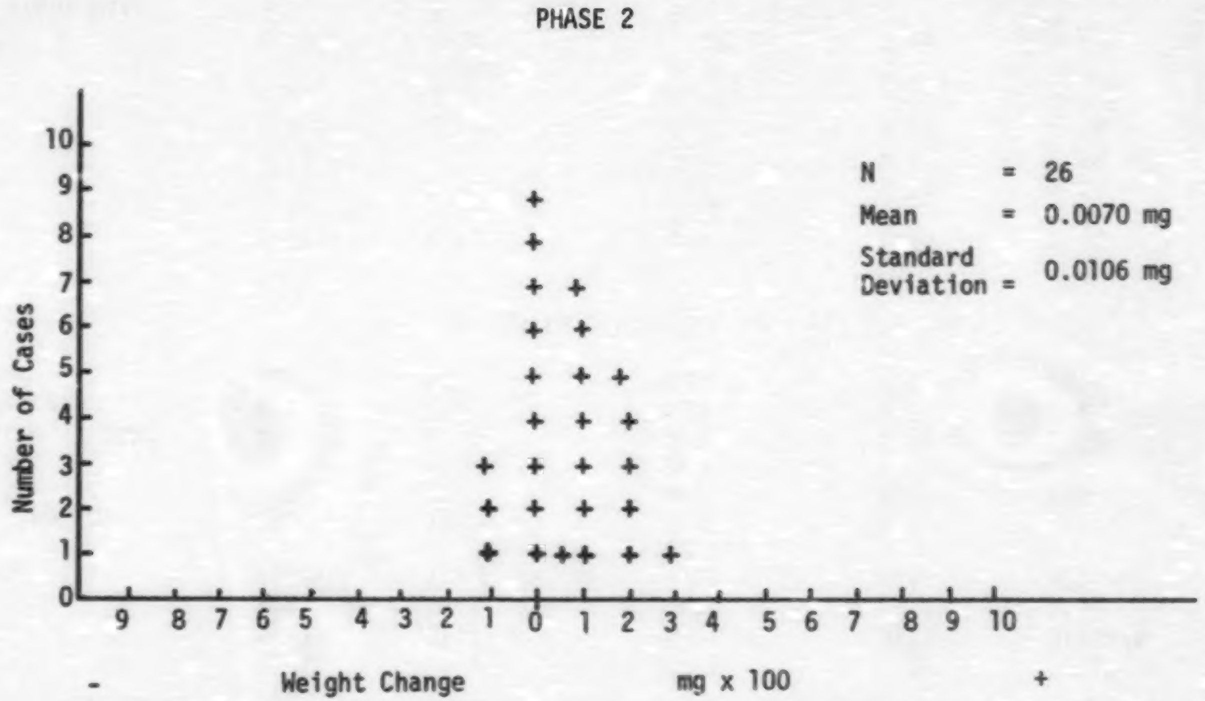

Figure 3. Weight change of control discs (gross weight 730 mg).

Table 1. BLANK IMPACTOR TESTS - PHASE I

Run No.	1	2	3	36	56[a]	63[a]
Date	9/17/75	9/17/75	9/18/75	9/23/75	9/25/75	9/26/75
Start Time	1152	1448	0810	0910	1550	1746
Substrate	Apiezon	111 Compound	Fluorolube GR 660	Apiezon	Apiezon	Fluorolube GR 660
Sample Time, min.	123	121	119	120	120	119
Temp., °F	253	253	256	260	210	218
Gas Volume,[b] DSM^3	2.94	2.85	2.75	4.12	2.52	2.84
H_2O, Percent	4.4	4.4	4.4	3.0	4.6	5.3
SO_2, ppm	600	640	630	750	750	680
			Change in Collection Disc Weights, mg			
Stage						
1	0.335	-0.02	-0.115	0.68	0.16	-0.23
2	0.135	-0.34	0.05	0.23	0.09	-0.17
3	0.27	-0.31	-0.075	0.15	0.06	-0.27
4	0.12	-.035	-0.21	0.03	0.05	-0.28
5	0.085	0.02	-0.17	0.09	0.05	-0.24
6	0.21	-0.38	0.22	0.00[e]	0.01[e]	-0.06[e]
7	0.01[e]	0.015[e]	0.01[e]	-0.01[e]	0.01[e]	-0.07[e]
Filter[d]	0.075	0.28	0.225	-0.49	0.23	0.25
No. of Blank Disc	6	6	6	5	5	5
Average Weight Charge of Blank Disc	0.192	-0.230	-0.050	0.236	0.0820	-0.238
Standard Deviation	0.097	0.108	0.160	0.259	0.0466	0.0432

a. boiler at half-load
b. dry standard conditions, 21.1°C, 760 mm Hg
c. corrected to 3 percent O_2 (preliminary results)
d. Reeve Angel 934 AH
e. controls

The results of the Phase II blank runs are shown in Table 2. The average weight increase for eight blanks was 0.05 mg. An analysis of variance reported in Table 3 indicated that the weight increases were random and not related to a specific parameter. Inspection of the plates revealed that some contamination existed--such as fibers from the inlet filter and fly ask.

One confounding aspect of these blank runs was the changes in stack temperature caused by load changes and ambient temperature (the stack is uninsulated). Smith et al (1975) found that the weight increases on blank runs were related to the stack temperature. However, it is suspected at the low temperatures under consideration, that this was not an important variable. It should be pointed out that these results are for a specific low temperature and low SO_2 source. The results cannot be extrapolated to other sources with the existing information. Additional field tests will be required to determine the changes in weight of blanks on other sources.

Additional improvements in procedures are planned--such as protection use of a portable clean bench.

CONCLUSIONS

The following conclusions are evident from the current study.

1. All aspects of the particle size measuring operation need to be considered for compatibility and efficiency. This includes the people performing the various duties, the impactors, substrates, balances, support equipment, and procedures.

2. The test plan should include both controls and blank tests to evaluate the quality of the data.

3. The results of the controls and blanks should be carefully analyzed to improve procedures of the impactor tests.

4. The average weight increase of 0.049 mg for eight blank tests should be considered specific only for the source tested. Evaluation of the Apiezon-L coated 316 stainless steel collection disc will be required for other sources.

ACKNOWLEDGEMENTS

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Table 2. BLANK IMPACTOR TESTS - PHASE II

Run No.	49[a]	50[a]	7	42	8	9	59	16
Date	11/14/75	11/14/75	11/17/75	11/11/75	11/10/75	11/10/75	11/13/75	11/12/75
Start Time	1000	1055	0925	0920	0800	0849	0936	0843
Substrate	Apiezon	Apiezon	Apiezon	Apiezon	Apiezon Mylar	Apiezon Mylar	Apiezon Mylar	Apiezon Mylar
Sample Time, min.	0	10	60	120	0	10	60	120
Temp., °F	205	205	240	210	230	230	220	220
Gas Volume,[b] DSM^3	0	0.182	1.17	1.86	0	0.155	1.07	1.82
H_2O, Percent	---	1.4	5.9	5.5	---	1.5	8.2	4.4
SO_2, ppm[c]	490	620	680	620	720	720	690	640
Weight Change, mg								
Stage								
1	0.08	0.31	0.05	0.13	0.02	0.05	-0.02	0.07
2	0.01	0.05	0.08	0.035	0.07	0.05	0.00	0.06
3	0.01	0.01	0.18	0.06	0.065	0.05	0.025	0.01
4	0.00	0.06	0.03	0.05	0.06	0.03	0.02	0.01
5	-0.01	0.01	0.03	0.03	0.03	0.03	0.04	0.15
6	0.00	0.03	0.03	0.03	0.03	0.09	0.50	0.03
7[d]	0.00	-0.01	0.00	0.01	0.00	0.02	0.01	0.02
Filter	+0.03[e]	+0.30[e]	0.09[e]	0.09[e]	-1.33[f]	-1.60[f]	-0.06[f]	-0.70[f]
No. of Blank Disc	6	6	6	6	6	6	6	6
Average Weight Charge of Blank Disc	0.015	0.078	0.067	0.056	0.046	0.052	0.114	0.0325
Standard Deviation	0.033	0.115	0.059	0.038	0.022	0.020	0.199	0.026

a. boiler at half-load
b. dry standard conditions, 21.1°C, 760 mmHg
c. corrected to 3 percent O_2 (preliminary results)
d. controls
e. Reeve Angel 934 AH
f. Whatman 41

Table 3. ANALYSIS OF VARIANCE OF PHASE II BLANK TESTS

Source of Variation	Sum of Squares	Degree of Freedom	Mean Square	F
Sample Time	240.77	11	21.89	0.030
Substrate	5.33	1	5.33	0.073
Residual	3163.37	35	90.38	1.24
Total	3409.48	47	72.54	

NOTE: The weight change in mg x 100 was used.

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DISCUSSION

SMITH: What would be a typical stage weight, that is particulate catch, at the outlet?

ENSOR: Well, probably on the order of a milligram or so depending on the stage. Again, I'm still screening the data, but we're getting on the order of a total of 5-10 milligrams.

SMITH: What did the deposits look like visually? Did they look as if the grease wicked up in the traditional fashion?

ENSOR: Well, that is an interesting thing. We found that what the grease does is extremely important. When you're very careful and paint it on, generally it doesn't cause problems.

SMITH: As far as particle retention, though, does it wick up through the layer of particulate?

ENSOR: I would suspect that that is not a significant thing. We've noticed that the deposits with this type of preparation are generally fairly fragile and may be self-adhering. But again, we're not using very heavy stage loading, and teis business of retention of greases is an interesting point. Tom Cahill has started using Apiezon-L with Lungren samplers and has done a fairly extensive retention study using trace elements as tracers, and he has found that this Apiezon grease is probably the most effective thing we've ever found. It's more effective than Dow Corning and in comparison to other materials. But I think, certainly, the efficiency with collection time is something that bears looking into.

HARRIS: You've had some indications of side collection on the mounds anyway with that Apiezon.

ENSOR: Yes, sometimes you'd see the mounds break, and in the collection it would look a little funny.

_____: What's been your experience with that electrobalance in transporting it around. Can you weigh something before taking it and weigh it again and be sure that the balance is correct?

ENSOR: Well, generally it has been pretty reproducible. We worked up some procedures on our own and felt that they weren't terribly satisfactory. I finally went to Colin Williams of Cahn and told him about the problem, asked him what would be the optimum technique for weighing these substrates, and he suggested weighing by substitution. We feel that it's working real well for us. Like any other equipment, you have to exercise an element of precaution and check it over.

CALVERT: On that point, we have one and we've had nothing but trouble.

MCCAIN: We've got and we've had very good success with them.

_____: Were those model 4100s?

MCCAIN: No, we've got a G-2, the next one up from that with a digital readout and then the automatic, and they've all performed highly satisfactorily, and they've traveled all over the country under airplane seats.

BYERS: Let me ask you this, if you had your choice at a field location, where you could use an analytical balance: in a laboratory or your portable electrical balance; which one would you use?

ENSOR: Probably the Cahn. You just don't find analytical balances in the field.

_____: To carry a microbalance around is just not practical.

HARRIS: Well, Seymour has had some success with this, carrying a microbalance around to use.

CALVERT: We carry both. We carry a Sartorious. The Cahn is great as far as not being susceptible to vibration, and when it works it's fine. But when it doesn't, there you are way out somewhere with no balance. We always have the Artorious as a backup, and we've had to use it many times. We've gone through the procedures that you found successful, and it didn't work on ours. I don't know what the latest is. We keep getting correspondence and new pieces to put in and try.

_____: What is your stage weight generally?

CALVERT: I don't know.

ENSOR: I think that thw size is very important.

CALVERT: They are not much different from yours.

HARRIS: I think that Colin is going to go into this a little bit more. One of the things that's going to come out is that, if you've got some idea of what your stage weight is, the electrical balance can be turned to that area. It is not really a linearly sensitive type of instrument, and you can tune it to a little bit finer degree at each end of the range. If you set it up that way, then you have better luck.

ENSOR: I think there is a kind of moral here. I'm kind of a do-all as both a manufacturer (of equipment) and a practioner. One of the most discouraging things to me is to go out and to talk to somebody and find out that they have had problems for years with a piece of equipment and they didn't say anything. I think that one of the most important things we do is to talk to Cahn to try to find out what's going on and to see if there is a better procedure. I think that this goes for all of the equipment that we use. The manufactures can build something that we can use once they know what we need.

HARRIS: One of the things we hope to do is to feed some of this back to the manufacturers and see what they can do. One of the things that has come out repeatedly is the problem of sampling points and how we're always not in an ideal position with respect to the diameters upstream and downstream and so forth. You might be interested in getting a copy of the report that Bill Kuykendal has had on contract from TRW on gas flow measurements, total volumetric flow. One of the things that is indicated there is that, for the gas flow itself, the closer we are to a controlled disturbance like an elbow, the better we understand the flow patterns. It may be a skewed profile, but it's defined. In a rectangular duct, you get a nice smooth profile--all of it sqished down to the bottom but it's there--and it is a nice regular thing. A few diameters downstream you may have eddies where the velocity varies widely. The problems that we have had in finding nice sampling positions may not have been that much of a problem. It hasn't been defined in terms of what it does to particles. But since many of the particles, especially those in the fine particle region, follow the gas flow streams anyway, it may mean that we haven't had that much of a problem. We may have been fortuitous in not being able to get to these ideal sampling positions.

BYERS: Well, there is a good paper, I can't give you the exact reference, but it's by Oxley, where he is taking velocity profiles and concentration profiles in ducts where he has been less than 1 equivalent diameter from a bend or something upstream and he showed what the concentration variation is at each equal area. I think that it is a very useful piece of information if you cannot get that ideal location.

HARRIS: Yes, an extrapolation of the material they have in the study is that for firms trying to get a measurement of total volumetric flow, without going to some major, elaborate thing, it's probably better at an elbow and use a single probe designed for that area, eliminating all of this multi-point sampling. It may get a better piece of information and we may end up with the same sort thing with dust particles.

BOLL: That means you have to make a presurvey of the points you're going to pick for it to be representative of the total.

HARRIS: Yes , that's right, you have to. But as far as I'm concerned, if you're going out on a field testing and you haven't made a presurvey, you've blown the first week anyway. You know there is no way you're going to go cold to the site and say, "gee, I've been to a power plant before and they won't give me any problems". They don't build two generators at the same power plant the same, much less two different plants.

BOLL: I agree with you, that's exactly right, up and down, through a duct, when you talk about small particles. It may even be true crosswise. However, when you're talking about 100 feet crosswise, I'm not so sure it's still true. Sometimes you can get different firing at this burner than at that burner and the gas flows are not uniform.

HARRIS: Yes, there are limitations to the idea, and I think that this is just one area where the presurvey comes in. You may be able to use only three probes down through there for the three burners and you don't have to do a 700 point traverse to try to establish a profile. All of this needs to be taken into consideration.

EXPERIENCE IN PARTICLE SIZING OF PETROLEUM INDUSTRY PARTICULATE EMISSIONS

R. L. Byers, Exxon Research and Engineering Company

HARRIS: Lee Byers of Exxon will go through some of his experience next.

BYERS: Particulate size measurements in the petroleum industry are of concern with respect to the three categories of particles. The first category is catalyst fines, which are emitted from catalytic cracking units; these particles are typically oxides of silica and aluminum. Coke fines (essentially 100-percent carbon), emitted from fluid coker units, represent the second category. The third category consists of fly ash from oil-fired furnaces and process heaters. The composition of the gas streams containing particulate matter from each of these categories is characteristic of most combustion gases: about 12 percent CO_2, 6 percent O_2, less than 0.5 percent CO, with the remainder of the gas being nitrogen and water vapor. The gas streams are typically 400° to 600° F and near atmospheric pressure.

I would like to share with you some of the experience we have had with various methods for sizing particles and indicate what we feel is a preferred approach for sampling and analyzing particles from the categories just mentioned

Size characteristics of catalyst fines have been of greatest interest since catalytic cracking units are one of the seven major stationary sources covered by EPA regulations. Figure 1 represents the results of about 30 different particle size measurements taken at three different refinery locations. The figure compares size distribution obtained by microscopic analysis of membrane filters and by inertial impaction. Microscopic analysis was undertaken to determine if the large percentage of particles below 1.0 micron, as measured by impaction, was real. This large percent (about 40 percent) of submicron material was not expected based on process considerations. It was felt that the high percentage of fines might be due to reentrainment in the impactor.

Before comparing these results further, I should indicate how the filter and impactor samples were taken. The impactor samples were taken with an Andersen impactor located in-stack, between the probe nozzle and

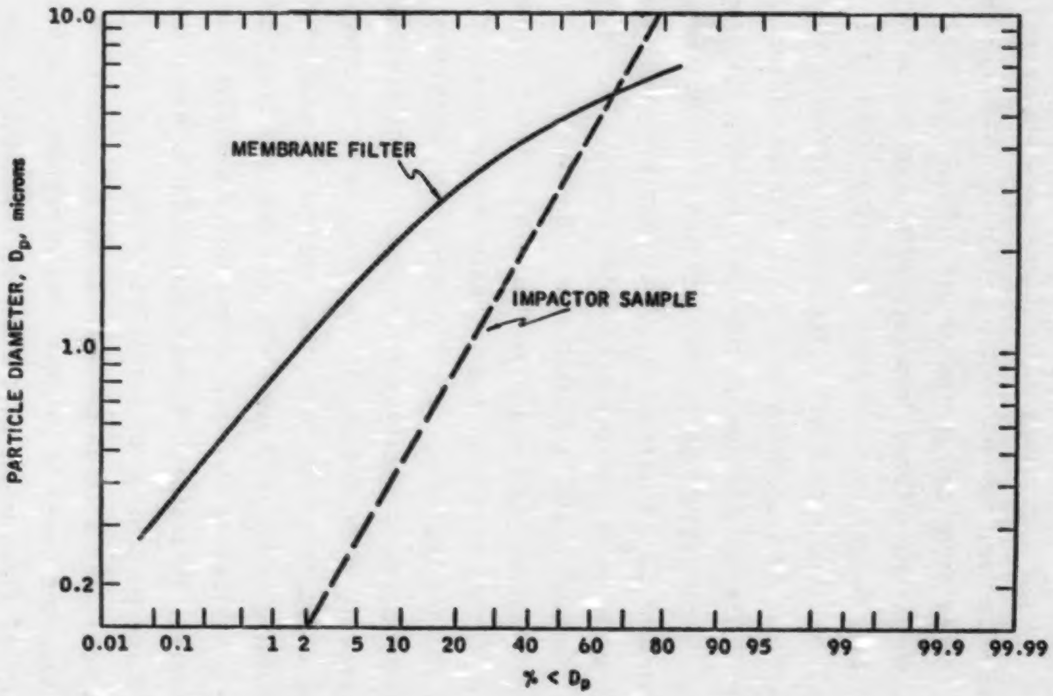

Figure 2. Fluid coker particulate emissions size distribution: impactor vs. membrane filter.

sampling probe of an EPA Method 5 train. No cyclone was used ahead of the impactor and, of course, there was no need for the heated oven of the Method 5 train. The Andersen plates were greased with a 2 percent solution of grease dissolved in benzene or toluene. The membrane filter samples were taken out-of-stack by replacing the cyclone, filter, and the heated oven by two 47 mm stainless steel filter holders, which attached directly to the end of the sampling probe. One of these filter holders contained a blank filter and was used to establish isokinetic flow through the sampling train. The second filter holder contained the filter which was used to obtain the sample. The membrane filter holders were heated; the sample filter was exposed for varying lenths of time in order to obtain samples which consisted of only a monolayer of particles.

As Figure 1 shows, microscopic analysis of membrane filters results in a signficantly different size distribution for catalyst fines than obtained by an Andersen impactor with greased plates. The microscopic results show a mass median diameter of about 2.5 microns, with about 5 percent of the particles on a mass basis being less than 1.0 micron; corresponding values for the impactor measurements are approximately 1.3 microns and 40 percent, respectively.

These results satisfied our hypothesis directionally, that is, that the impactor was undersizing the particles. More support was found when the size distributions from the two sampling methods were plugged into a mechanical model we have developed for predicting the fractional collection efficiency of jet ejector wet gas scrubbers. The model contains a correction factor which only has physical meaning when its value falls between 0 and 1.0. When using membrane filter data along with scrubber performance data, realistic values of this constant were found. Using impactor size distributions resulted in values of the constant which exceeded 1.0. Although this in itself may not prove that membrane filter size measurement is more representative of the true particle size then impactor measurements, it does tell us that, if we want to predict scrubber performance, either for purposes of design of new scrubbers or optimization of existing scrubbers, we should use membrane filter particle size data.

ENSOR: It seems like you are comparing two different types of diameters--one a D_{50} diameter and the other some kind of aerodynamic diameter.

BYERS: The diameters shown in Figure 1 are the physical diameters of the particles for both sizing methods. The microscopic sizing is done by comparing the projected area of a single particle to the equivalent area of a circle. This is an actual physical measurement. In the case of impactor sizing, since the density of the catalyst is uniform and well known, it is possible to calculate the actual physical D_{50}. Thus, the two size distributions are comparable since they are both measurements of the same property, the physical diameter of the particle.

SMITH: Did you do a microscopic analysis of the impactor catches?

BYERS: We looked at the individual impactor stages microscopically and confirmed that there were larger particles on a given stage than would be expected based on the impaction equation, when a value of the impaction parameter for a collection efficiency approaching 100 percent was used. We did not determine a size distribution analysis per stage, however, since there were too few particles present to give a statistically reliable particle count.

MCCAIN: If you eliminated the backup filter from your percentage distribution on your impactor, what would happen?

BYERS: On the backup filter we have about 10 percent of the total weight collected in the impactor.

MCCAIN: That would not pull the curves (in Figure 1) much closer.

CALVERT: Do you have uniform distribution across the membrane filters?

BYERS: Yes. We look at filters with different exposure times and choose a filter on which the particles are not agglomerated and where the distribution is uniform. Some filters do have high concentrations at the center of the filter; these were not used for size analysis. High concentration of particles at the center of the filter can be minimized if the probe is kept short and if it is cleaned just prior to taking the membrane filter sample.

RAO: Were the particles spherical?

BYERS: No, they tend to be irregular in shape but more spherical than needle-like.

Figure 2 shows that comparisons between membrane filter and impactor size distributions for coke particles were quite similar to those found for catalysts fines. For coke particles the membrane filter data give typical mass median diameters of about five microns with about two percent less than 1.0 micron. Corresponding values frr the impactor data are 2.6 microns and 20 percent.

As implied earlier, comparison of membrane filter data with impactor data is only meaningful when the particulate matter is of uniform density and the density is known. Figure 3 is an example of how one can be misled if he uses a membrane sample to size particles of nonuniform and unknown density. The figure shows typical size distributions of fly ash emitted from oil-fired boilers. The data were taken for two fuels: fuel A,a "dirty" (high Conradson Carbon Number) fuel, and fuel B, a relatively clean fuel. There the differences between the mass median diameters measured by the microscope vs. impactor ranged from five to ten fold. This is a significantly greater variation than was obtained by the two sizing methods when the particles were catalyst or coke fines. The obvious explanation is that fly ash contains a large number of hollow particles which, when counted as particles having the same density as solid particles, results in a size distribution biased toward large particles. In fact, the microscopic and impactor size distributions of Figure 3 are not even comparable. Only when particles density is uniform and known is it possible to meaningfully compare the two size sampling methods.

Much has been said earlier today concerning the use of various substrates when collecting a sample by means of an impactor. We have done a number of measurements to assess the relative effectiveness of grease vs. paper substrates in reducing bounce and reentrainment. Figure 5 shows results of instack Andersen samples for both grease and paper substrates. The paper substrates show about a 50 percent increase in mass median diameter and about 10 to 15 percent less mass smaller than 1.0 micron

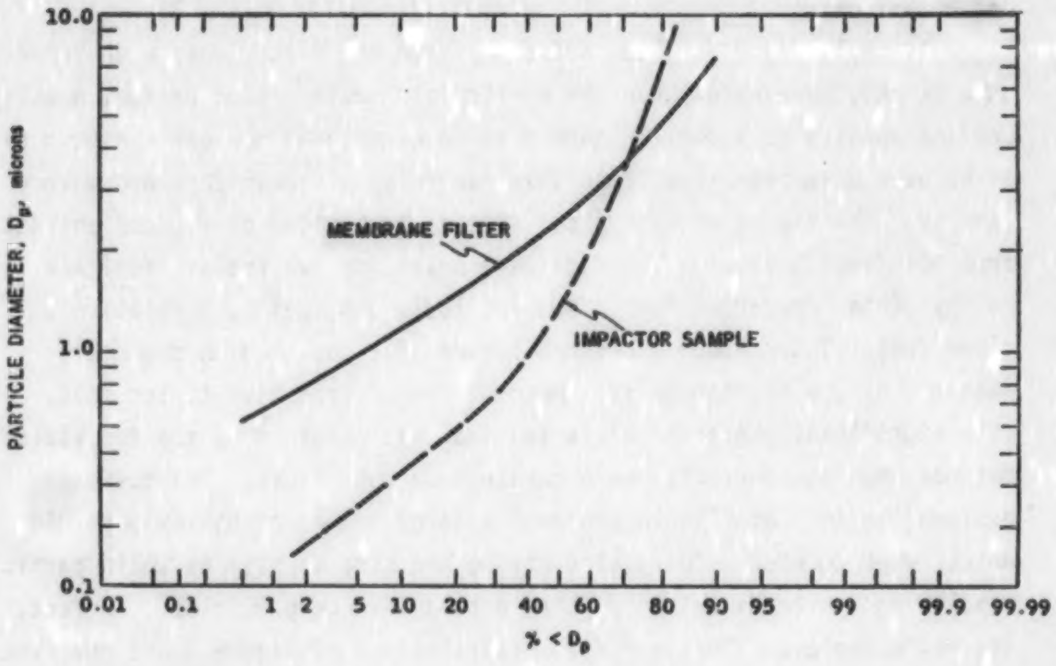

Figure 1. Cat cracker particulate emissions size distribution: impactor vs. membrane filter.

than found when using grease substrates. When these substrates were compared for out-of-stack samples, similar results, shown in Figure 6, were obtained. The overall size distribution for the out-of-stack samples was smaller, however, for both sampling methods due to the loss of the larger particles in the probe. This latter effect was confirmed by the data shown in Figure 7 which compares in-stack vs. out-of-stack particle size measurement using paper substrates.

On the basis of the results we have obtained to date, we find paper substrates are preferable to grease in obtaining reliable size information. Paper substrates also have the advantage of being stable at temperatures up to 900° F; most greases are not stable above about 400° F.

ENSOR: Were you running replicates of these tests?

BYERS: Yes. The data shown in Figures 5, 6 and 7 are for single tests, however.

ENSOR: Single tests? You do not know what your variation was between identical tests?

Did you say that the difference between these different curves is a difference in firing conditions?

BYERS: The different curves for any given figure represent data taken at constant firing conditions. The difference in the curves shown in Figures 4 and 5, however, is due to a change in firing conditions betwen those two tests. Both curves in Figure 4 were at the same firing condition; likewise for Figure 5.

RAO: When you collected the sample, did you put the probe in the same location for in-stack samples?

BYERS: Yes.

All of the sampling work I have just described was done under typical stack conditions of 1 atmosphere pressure and 300°-600° F. In one of our test programs it was necessary to sample a process stream, upstream and downstream of a particle collector, where the gas pressure was three atmospheres and the temperature was about 300° F. Seymour Calvert was talking

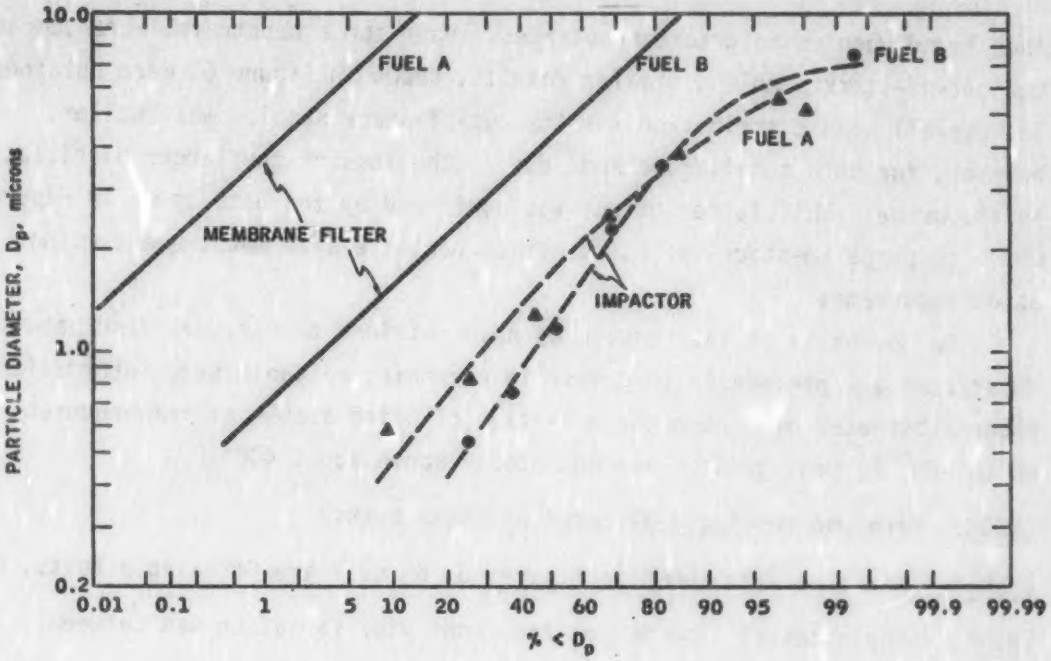

Figure 3. Oil-Fired Boiler Fly Ash Size Distribution: Impactor vs. Membrane Filter.

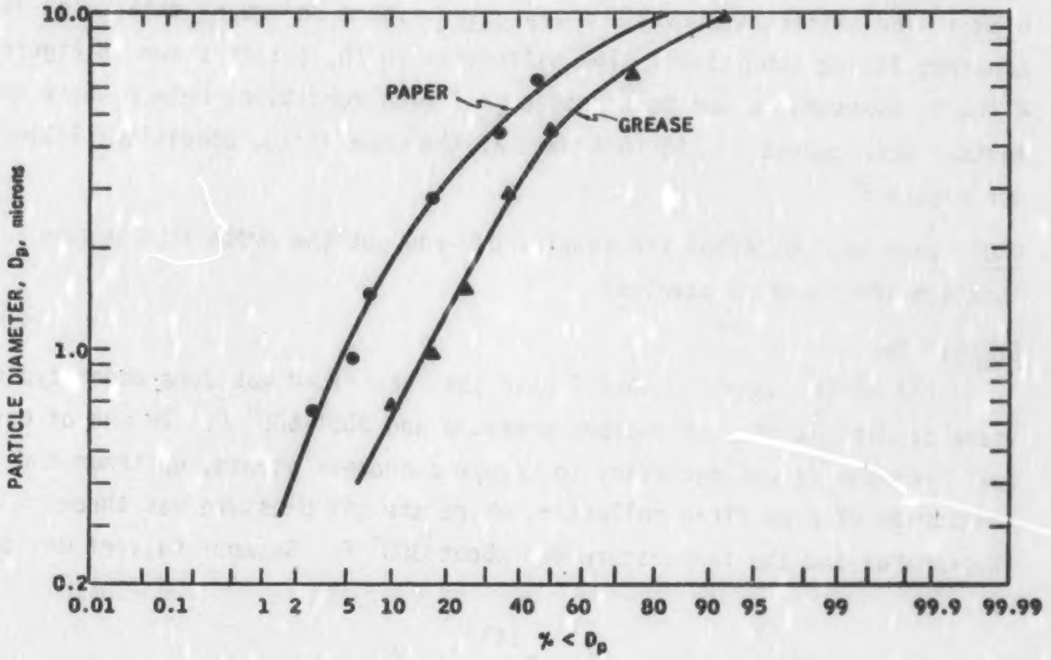

Figure 4. Oil-Fired Boiler Fly Ash Size Distribution: In-Stack Impactor, Grease vs. Paper Substrate.

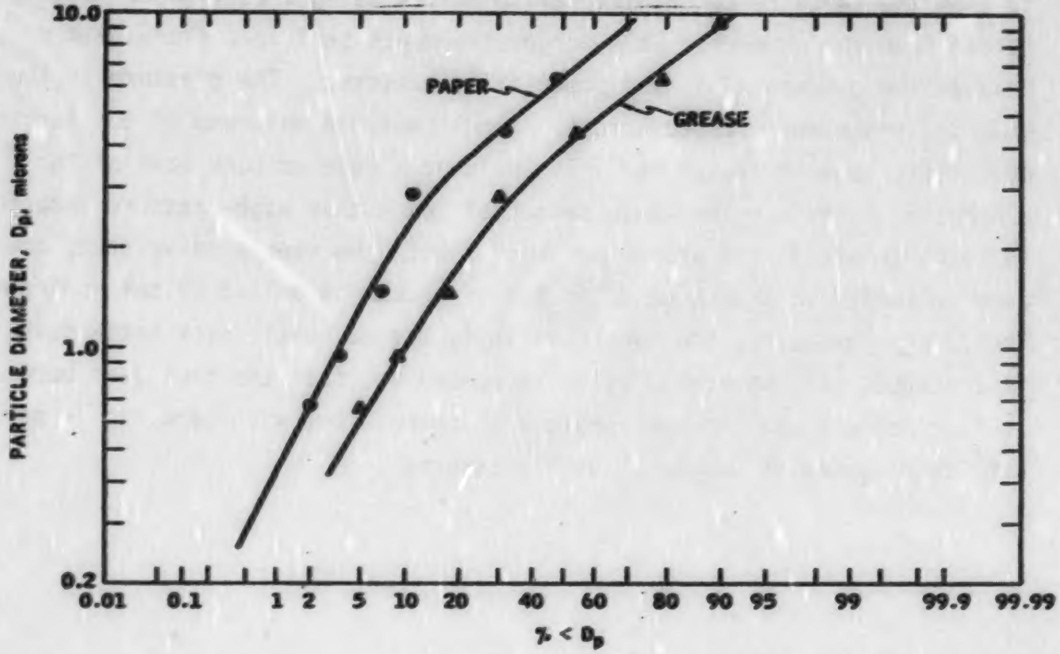

Figure 5. Oil-fired boiler fly ash size distribution: in-stack impactor, grease vs. paper substrate.

this morning about the CO content in certain gas streams to be sampled. In our process stream we had 5 percent hydrogen, about 7 percent CO, 1000 ppm H_2S. We wanted to sample this stream for both particle concentration and size. The sampling port we used to do the job is shown in Figure 8.

A pressure chamber 10 inches in diameter was designed. It was made that big merely to accommodate an Acurex stainless steel filter holder. Figure 8 shows an Andersen sampler in place in the port. We were able to keep the particle collection device hot by using a continuous purging stream from the chamber. This purge stream was sent to a flare header because the gas was toxic and potentially explosve. The pressure in the sampling probe was dropped across a needle valve downstream of the particle collector. Downstream of the valve we used a regular back half of the EPA Method 5 train. The whole secret of doing this high-pressure sampling is to do things in the proper sequence. With the needle valve shut, the pump is turned on and about 20 inches of vacuum is pulled on the impingers. Then, very gradually, the needle valve is opened until isokinetic sampling is achieved. If the needle valve is opened too fast the impingers become pressurized and the impinger seals are broken. But with care, it is possible with this system to sample at high pressures.

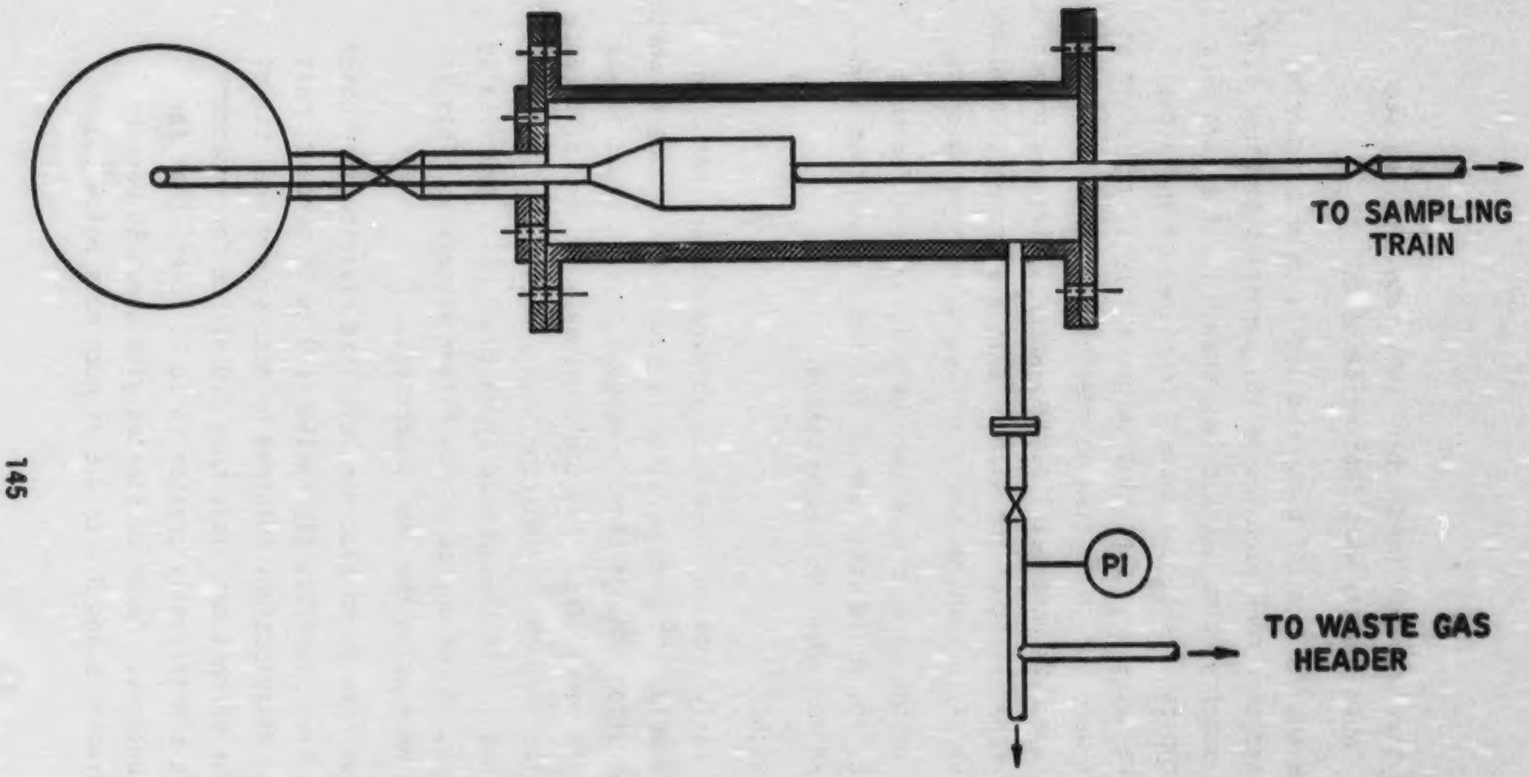
TO SAMPLING
TRAIN
PI
TO WASTE GAS
HEADER

Figure 8. High pressure sampling port.

DISCUSSION

BOLL: Lee, you said that when you used a bare substrate, you had seen evidence of bounce. Could you say what that evidence is?

BYERS: Yes. We took the whole metal plate and put it under a scanning electron microscope and then took pictures of the particles at about 5,000 X magnification. The sampling time was not long enough for the particles to pile up into the typical peaks; the deposit consisted of individual particles. There were not enough particles to get a size distribution on each stage, but there were enough to give an idea of the range of particles that landed on the stage. On bare metal plates, where a five micron particle should have been the upper limit of the size of particle present, based on 100 percent collection efficiency, we saw particles up to seven microns.

BOLL: If efficiency of the upper stages were only 95 percent for that seven micron particle, you would still expect to find some of them downstream. But you found more than that expectation.

BYERS: Yes, that's right.

HARRIS: Some of the early work on the Brink, looking at bare metal versus greased substrate and using the backup filter as your means of seeing whether you are getting penetration through these. Because in this case, it was fairly large fly ash we were using. You could visually see the difference between what penetrates through the impactor back stages to the filter and what doesn't. That is, in the greased stage there was a practically white filter, and, say, in the ungreased the filter was dirty. This is meaningful since you were using the same instrument.

BYERS: In summary, we have found that particle size distributions based on cascade impaction have significantly smaller (40 to 50 percent) mass median diameters than distributions obtained by microscopic analysis of membrane filters. The percent mass less than 1.0 micron for impactor size distributions is significantly greater (5 to 10 times) than for membrane size distributions. Paper substrates give about 50 percent larger mass median diameters and 0.1 to 0.5 as much mass below 1.0

micron as grease. When sampling conditions permit, membrane size distributions are recommended for use in calibrating impactor size distributions. Paper substrates are recommended over grease substrates and membrane filters when impactor sampling is required due to temperature limitations or the presence of nonuniform density particles.

"HOW TO WEIGH IT, ONCE YOU HAVE COLLECTED IT!"

Dr. Colin J. Williams, Cahn Instrument Company*

HARRIS: Another important consideration in the collection of airborn particulate matter is the precision and accuracy of the weighing procedure. How do you weigh the material once you've collected it? We have invited Dr. Colin J. Williams, an employee of a major microbalance manufacturer, to speak on this subject and to tell us how to minimize our weighing problems.

WILLIAMS: Thank you, Bruce. In his introduction this morning, Bruce Harris stated that, "Most of the people here have been active in in-stack sizing problems" Well, that does not include me! My only involvement in this field results from working with people such as Dr. Ensor of MRI and Dr. Calvert of APT, and talking to others about their particular application problems, and reading a few reports published by EPA and SoRI. I do feel, however, that your weighing problems are basically no different from those of anyone else. The concerns of a microbiologist weighing the eyeballs of a plankton are the same as the in-stack particle technologist weighing an impactor plate loading, and that's what i'm here to talk about.

Let me first of all define the problem as I see it. There are various designs of cascade impactors available commercially, as well as others still in the research stage. Collection plates or substrates of various configurations exist, varying widely in weight and physical size. The typical sample loading is very small with respect to the weight of the impactor plate. A suitable balance must, therefore, possess adequate specifications such as load and sensitivity, and adequate features such as a large weighing compartment and large stirrups to support the plates. There is one other most important criteria which I will introduce in the form of a quotation from a paper by Messrs. Byrd, McCain, and Harris (1973): "...balances that operate well in the laboratory environment at required sensitivity, are not uncommon. However, the choices are severely limited when the balance must be transported to remote field test sites, set up in an industrial environment and still be expected to perform to the required precision. Those balances that are readily transported and have the necessary sensitivity generally have a maximum capacity of only a few grams." An accurate assessment indeed, and one that probably resulted in all heavy impactor plates being discarded in favor of light weight designs. I believe the heaviest are of the order of 1.2 grams. The heavier they are, the more unsuccessful one will be in measuring a small weight difference (between the unloaded and loaded plate weights).

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*Presently with Perkin-Elmer Corporation.

So, although the semimicro balances of Sartorius and Mettler serve the purpose adequately in a conventional laboratory (presuming their delicate knife-edge suspensions are protected from deleterious vibrations), their inability to be transported with ease render them quite unsuitable for transportation and field use in mobil labs or motels. Only electronic microbalances, with their totally different beam and sample suspension designs, can withstand the rigors of field use. The manufacturers of such microbalances rather appropriately offer foam lined metal carrying cases. The decision to use electronic microbalances is almost universal amongst impactor technologist. Maximum permissible sample weights of up to five grams and sensitivites down to 0.1 micrograms mean that they provide adequate specifications.

The accuracy of any sample weighing depends on various factors such as calibration stability within the electronics of the balance (this should be carefully checked during the evaluation of a microbalance); accuracy of the digital readout display (commonly ± 1 count on a ± 20,000 count display); and accuracy of the calibration weights (class M weights, traceable to the National Bureau of Standards, are typically better than 0.05 percent accurate). An additional burden of responsibility lies with the calibration weights when they are used in substritution weighing. It is usually safe to say that accuracy is not a limiting factor in a weighing procedure. Precision is far more important.

Is substitution weighing in fact necessary or even desirable? The answer is yes. Such a technique allows weighing of samples to a finer resolution than when one chooses a weight range large enough to display the full sample wight. Most microbalances offer a series of weight ranges, including 0-1,000 mg, which has a resolution of 0.1 milligrams. The range is large enough to exceed the exeed the weight of, for example, a 732.1 mg impactor plate, but the resolution obtained is insufficient, then, considering the total wight of sample collected. If the following substitution procedure is used, resolution to 0.01 mg can be obtained. Set the weight range to 0-200 mg, for example; place 600 mg of calibration weights (preferably a 500 mg and 100 mg weight) as substitution weights, on the sample weight tray; and counterbalance (tare) with 600 mg on the tare weight tray. Zero the readout display with the electrical zeroes.

Remove the substritution weights from the sample weight tray and replace them with the impactor plate. Considering the relatively heavy weights of these objects, remove and replace with great care. Do not jab downwards with forceps when attempting to pick weights off the weight tray and do not drop weights onto trays. The readout should now display something like 132.13 mg, which, added to the substitution weight of 600 mg, means the impactor plate weight is 732.13 mg. With great care, this procedure could be taken a step further, using substitution weights totaling 720 mg and weighing on the 0-20 mg weight range where the resolution or readibility is 0.0001 mg. An alternate approach as far as tare weights and substitution weights are concerned is to use blanks (impactor plates). Then, the bouyant forces on the sample and counterweight, and on the substituted plate and actual sample plate, will be the same.

Precision or repeatability is the most important figure of merit for any microbalance. If a sample weight reading cannot be repeated within certain performance specifications, it was not worth the effort of weighing it in the first place. Precision, expressed as a standard deviation, is actually based upon 11 consecutive weighings. The balance manufacturers' literature should be consulted and the best precision figures (best that can be expected) calculated and compared to actual results. Strictly speaking, only when such an exercise results in weight values for which the calculated precision equals, or is better than, rated precision for the chosen sample weight and weight range should one assume one has an acceptable, repeatable, and accurate weight.

Good weighing technique is desirable for accurate weighings, but no special skills are required other than an understanding of the various weighing steps and a certain amount of practice. Of course, not even those highly skilled in the art of microweighing can expect success unless the conditions are suitable. Dr. Ensor experienced severe problems during an early field test until he realized that he was scuffing his shoes on the nylon carpet of the motel-laboratory. Severe electrostatic charges can easily be transferred to the microbalance weighing mechanism across the surface of the operator's body. In such instances, a radioactive ionizing source such as Polonium-210, an alpha emitter, (manufactured for example by Nuclear Products Company, South El Monte, California) is highly recommended. It should be placed on the floor of the balance weighing compartment throughout the weighing procedure. For impactor collection surfaces made of membrane filters, such as those of Sierra Instruments (Carmel Valley, California), it would also be advisable to momentarily hold the ionizing unit close to each side before weighing.

The topic of sufficient roominess of the weighing compartment when weighing large diameter foils has already been touched on. However, there is one other point I would like to make. In a recent report by Bruce Harris, the statement was made: "(It is desirable) to have a balance with a large enough weighing chamber to accomodate the "Pilat" foils without having to fold them." I agree completely. Even slight bending of metal foils incurs the risk of chipping off some dried particulate matter, as well as increasing the chance of contamination by touching the foils.

And may I quote again: "...insensitive to vibration, if it (the microbalance) is to be used in the field." The phrase "insensitive to vibration" is ambiguous and often misunderstood. A microbalance is perfectly capable of withstanding the rigors of transportation without any arrestment of the beam or suspensions (this is not the case, of course, with

with conventional beam balances as stated previously). No damage results when a microbalance is transported to a field test site, mobile lab, or motel, and set up within a few minutes by the technician. However, if the environment is inherently noisy with low frequency vibrations (from air conditioners in a mobile lab or from an industrial plant nearby), weighing could be seriously hampered. A simple test involves placing one's fingertips lightly on the balance table or bench. If vibrations can be felt, somewhat like a pulse, then probably a new, less noisy, environment will need to be located. In this context, a microbalance quite obviously is sensitive to vibrations even though it will not suffer damage as a result of them. The rule is to choose as sturdy a support table as is available, one that is apart from disturbances which might develop inertial forces in a sample during weighing and lead to slow and nonprecise weighing.

In conclusion, I wish to make the claim that some modern electronic microbalances can provide the answers to the impactor technologists' weighing problems. Considering all of the information I have acquired relating to your weighing needs, I do believe that sufficient capacity, sensitivity, accuracy, and precision are attainable, presuming you follow the recommended procedures. Given the opportunity, it's always possible that a tune-up job can be done to your microbalance, by a skilled service engineer, of course, and then the performance could possible exceed quoted specifications.

Gentlemen, thank you for the opportunity of participating in this in-stack particle sizing seminar, and I trust everyone now feels confident enough to return to their labs to accurately weigh the eyeballs of a plankton!

DISCUSSION

MCCAIN: We alleviated our problem at SoRI in handling large substrates, particularly those from the University of Washington, in the following manner. Their roils are rather stiff and probably could not be flattened for reuse if they were folded or bent to facilitate weighing. So a chamber was designed and positioned underneath a Cahn balance. It was necessary to remove the weighing mechanism compartment from the body of the balance and drill a hole through the base of this metal can. Another hole needed to be drilled through the balance chassis, and both of course to support the foils, and since the new weighing chamber was constructed of clear plastic, the operator could see if the stirrups had stopped swinging about before taking a weight reading.

WILLIAMS: Apparently SoRI is quite happy with the modifications that Joe McCain has just described. However, they are not modifications that most users would welcome undertaking, for fear of damage to an expensive microbalanced. Joe also tells me that the present setup is hardly suitable for field use, since a lack of rigidity in the added weighing chamber and the increased distance between suspended foils and their point of suspension to the beam result in significant pan swing. This problem presents itself in the form of increased weighing time and reduced precision.

Weighing chamber size and other features are not the same for all microbalances. The user is advised to measure his specific needs against a balance's specifications prior to choosing one.

CASCADE IMPACTOR DATA FOR ELEMENTAL ANALYSIS

T.A. Cahill, University of California
Presented by D.S. Ensor, Meteorology Research, Inc. - Co-Author

HARRIS: Tom Cahill of Crocker Labs cannot be here so Dave Ensor will present his talk. Crocker Labs is using cascade impactors to get data on elemental analysis.

ENSOR: Thank you. The elemental analysis of cascade impactor samples using ion-excited X-ray analysis (IXA) was summarized. The IXA system and the results of interlaboratory comparisons were described. The elemental penetration as a function of particle size through a scrubber was used as an example of practical application.

ACKNOWLEDGMENT

The scrubber results were obtained under EPA Contract 68-02-1802. The analytical system has been developed and used with the support from the California Air Resources Board, National Science Foundation, Research Applied to National Needs; and the United States Navy.

This paper was written with funds from Meterology Research, Inc.

INTRODUCTION

The desirability of having information on the elemental content of particulates as a function of aerodynamic size has long been accepted by scientists concerned with the biological and physical impacts of atmospheric aerosols. However, particle sizing devices deliver little material for subsequent chemical analysis, so that difficulties are presented in the generation of such information. Use of destructive methods, such as wet chemistry, atomic absorption spectrophotometry, emission spectroscopy, etc., compound the problem when information on more than a few elements is needed.

ELEMENTAL ANALYSIS SYSTEM

Recent advances in the accuracy and sensitivity of nondestructive X-ray-based analytical procedures have greatly alleviated these problems. In particular, energy dispersive X-ray spectrometers have allowed scientists to analyze for dozens of elements at one time from a sample of total mass of 1 mg, and extension to all elements sodium and heavier in a single analysis has been accomplished. Surprisingly, costs have also been reduced at the same time.

These methods however, have, limitations inherent to any X-ray based technique and limitations unique to energy dispersive systems:

- Light elements are difficult to make quantitative due to particle size effects and filter enmeshment effects because of X-ray attenuation. These mainly affect elements lighter than about potassium for typical cascade impactor samples.
- In energy dispersive systems, the necessity of seeing all elements, a great advantage, results also in difficulties in seeing weak elements in close proximity with abundant elements (ratios of greater than 1,000 in mass). Interferences cause problems for the complicated samples seen in stack effluents, although with care they are rarely serious and generally limit only a few elements.

Once these limitations are understood, these methods posses enormous advantage in that they allow one to gather great amounts of information in short sampling times with no predisposition about which elements are present and in what amounts. The nondestructive nature allows reanalysis to higher sensitivity precision when the preliminary data are interesting, an important advantage.

At the Crocker Nuclear Laboratory of the University of California, Davis, these methods are being applied to samples collected by Meteorology Research, Inc. Ion beams are used to excite the X-rays, which allows one to perform an analysis of a single deposit from an impactor with no loss of sensitivity. Any element sodium or heavier can be seen if present, although automatic reduction codes normally only print out 40 elements, ignoring all rare earths and most actinides. The most important information gained is often that some element is not present (to a stated level), a result fervently to be desired in many cases.

A schematic of the ion-excited X-ray analysis (IXA) system at the University of California at Davis is shown in Figure 1.

An 18 Me V alpha beam from the cyclotron passes through remotely readable graphite collimators and impinges on the thin target which was mounted at an angle of 45 degrees to the incoming beam. The sample or target is mounted into a 35 mm slide. The target slide changes is operated under real time computer control. Beam spot uniformity is acheived by the use of a diffusion foil (6.4 μm Al) and different sized target collimators. The beam is then collected by a Faraday cup and integrated to a precision of about 2 percent to give the total charge, Q, that passed through each sample. X-rays that pass through an active filter and a 25 micron Be window are converted into electrical pulses by a 10 mm^2 x 3 mm liquid nitrogen-cooled Si (Li) detector and associated pulsed optical feedback circuitry. Data are accumulated in a Digital Equipment Corp. PDP-15/40 computer with Nuclear Data 2200 Analog-to-Digital Converter's integral to the system, giving a spectrum of characteristic X-rays and a smooth background.

An example of an X-ray spectrum is shown in Figure 2, with a small Cu peak next to a Zn peak of 18 mg/cm^2. This system has mean accuracy in all interlaboratory intermethod comparisons for all elements Al and heavier of 1.03 ± 0.09, since January 1973. Over 30,000 analyses have been made to data, yielding over 5000,000 positive elemental determinations and very many upper limit determinations. The automatic data reduction procedure is shown in Figure 3. A nice feature is that any sample, no matter how lightly or heavily loaded, is awlays run at optimum count rate for the detector, as essentially unlimited X-ray excitation is available from the accelerator. Also, elements hydrogen through fluorine are not being routinely extracted from ambient air samples (with elastic alpha scattering). An example of the alpha spectrum is shown in Figure 4 of NaCl on mylar although this is not being done for stack samples at this time.

The system accuracy has been evaluated in a recent interlaboratory comparison reported by Camp et al (1975). The results are summarized in Figure 5 for standards made up of trace elemental solutions deposited in a filter. Each square is one laboratory, one method. The value on the

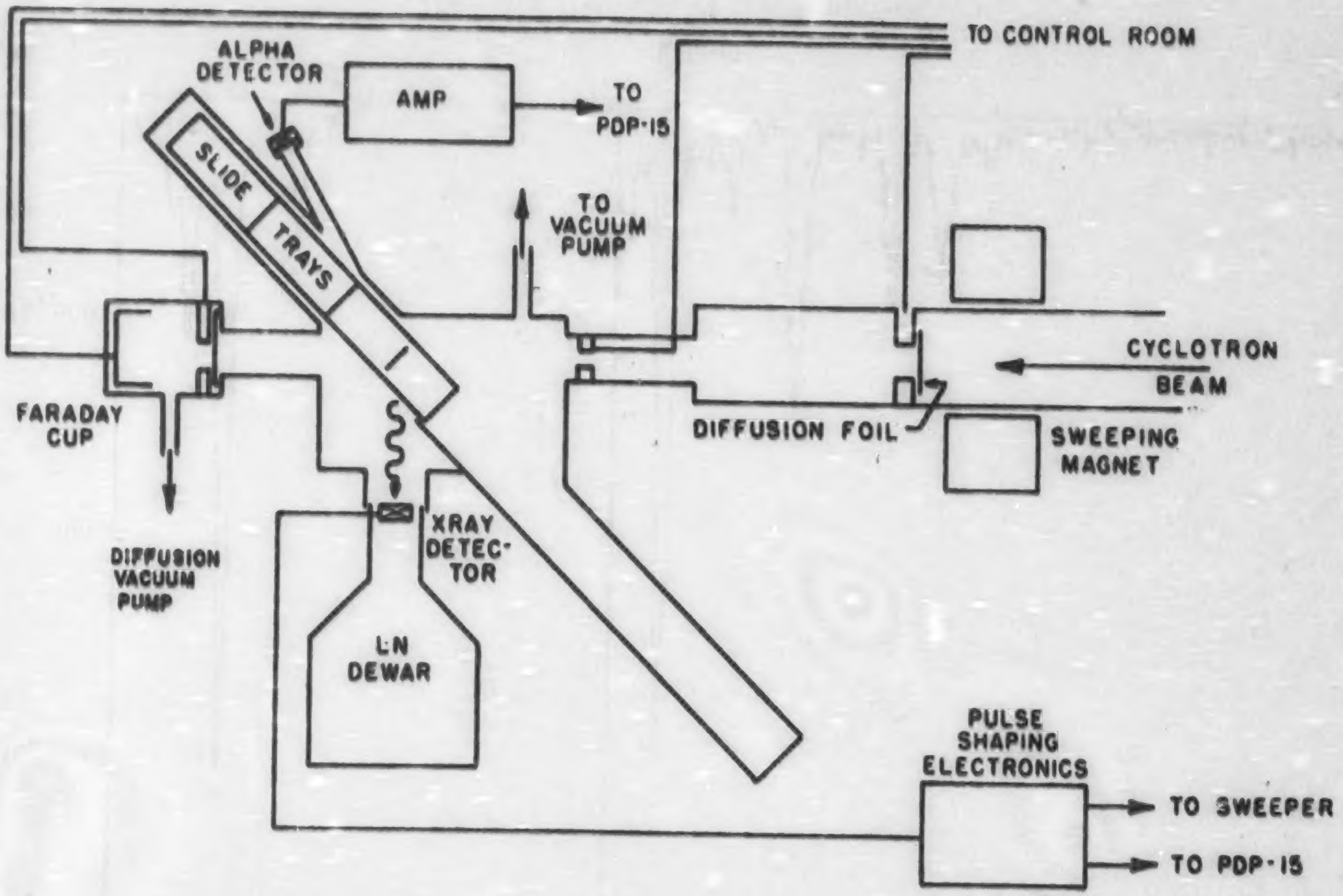
TO CONTROL ROOM
ALPHA DETECTOR
AMP
TO PDP-15
SLIDE
TRAYS
TO VACUUM PUMP
CYCLOTRON BEAM
DIFFUSION FOIL
SWEEPING MAGNET
FARADAY CUP
DIFFUSION VACUUM PUMP
XRAY DETECTOR
LN DEWAR
PULSE SHAPING ELECTRONICS
TO SWEEPER
TO PDP-15

Figure 1. Ion-excited x-ray analysis system at the University of California, Davis.

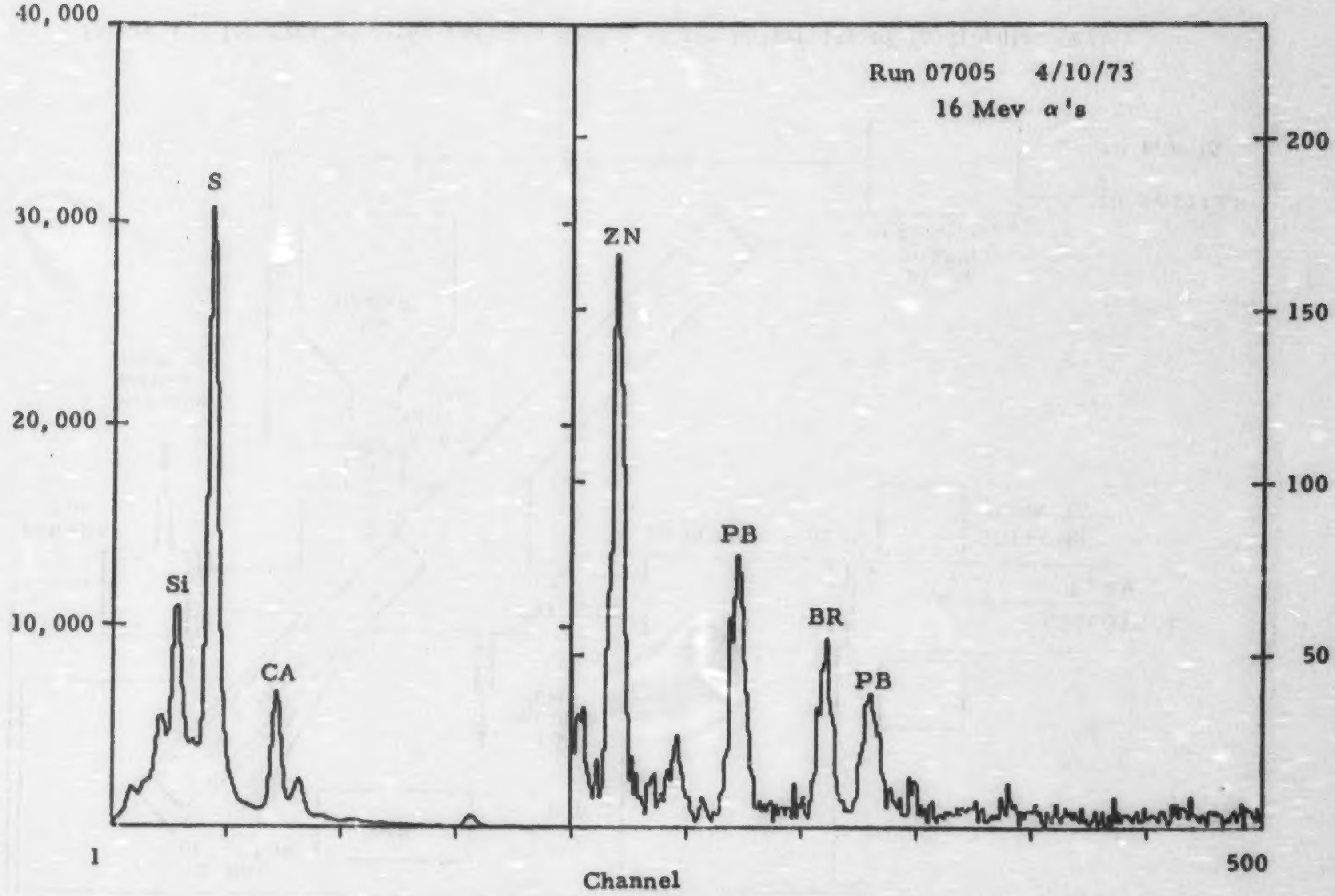

Figure 2. Example of x-ray spectrum.

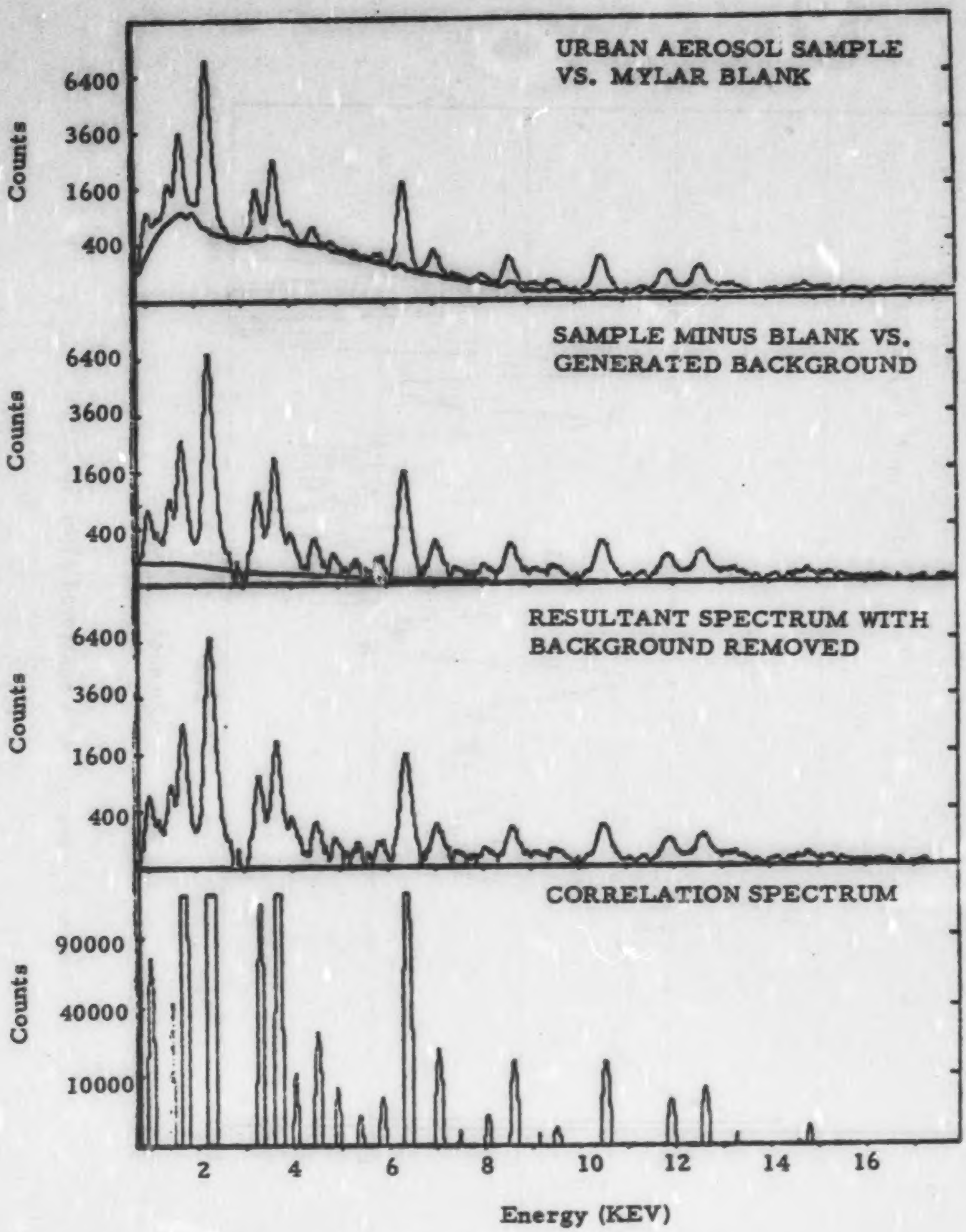

Figure 3. Example of automatic readout system.

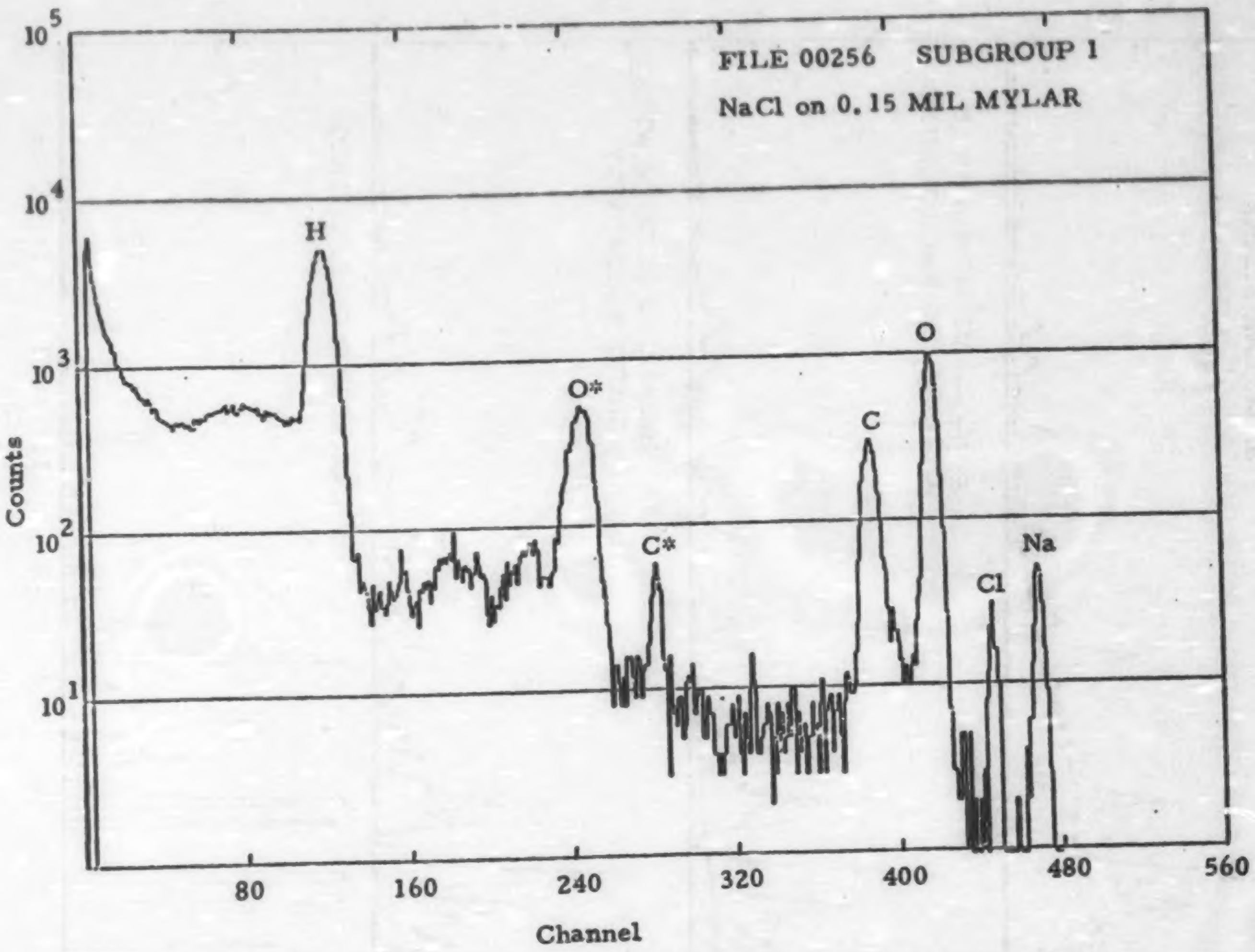

Figure 4. Example of a scattered alpha spectrum.

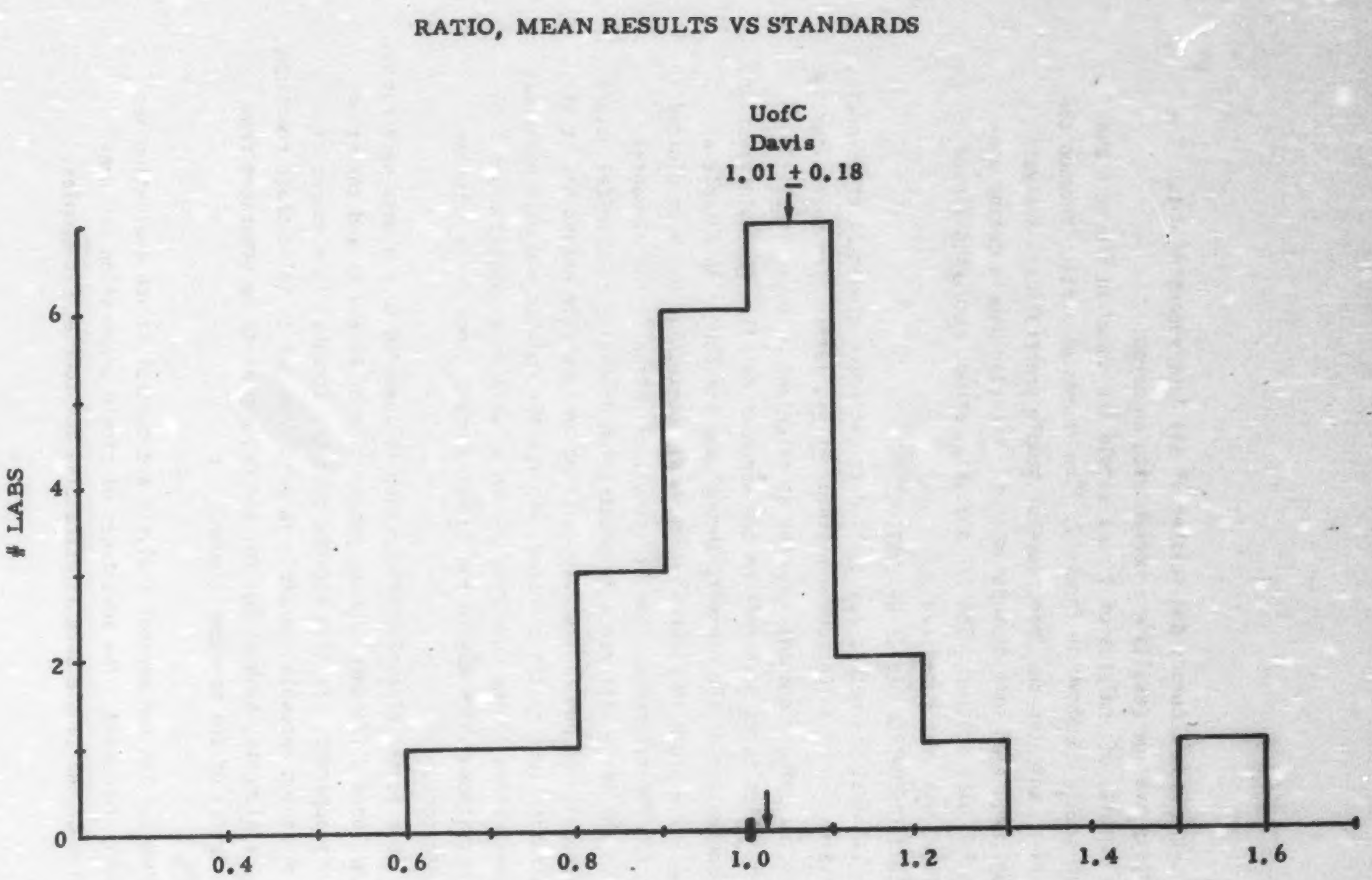

Figure 5. Results of interlaboratory comparison with solution standards.

figure is the mean accuracy and scatter of all labs reporting data. The UCD analysis-average results are marked with an arrow.

The results of analysis of a rock sample are shown in Figure 6 and a fly ash sample is shown in Figure 7. The increases scatter between the laboratories indicates the importance of sample preparation. Analysis techniques requiring some disruption of the granular sample during preparation had wide scatter. The IXA analysis method, requiring little preparation, was very consistent

APPLICATION TO CONTROL DEVICE EVALUATIONS

The IXA analysis method was applied to determine elemental efficiencies in the evaluation of a TCA scrubber (Ensor et al, 1975). The source testing was conducted using a scacade impactor as described by Ensor et al (1975). Inlet and outlet tests were made on the scrubber to allow the determination of the penetration of the elements through the scrubber. An example of the results are shown in Figure 8. The mass penetration has been plotted on the graph for reference. The striking implication of the elemental results is the large difference in penetration depending on chemical nature of the aerosol. Si and Al probably existing as insoluble oxides have a very low penetration through the scrubber, whereas the soluble elements have much higher penetration in the submicron region as well as a suggestion of a bimodal penetration, which may be due to entrainment through the scrubber mist eliminators.

In Table 1, the elemental penetration is compared to the mass penetration. Again, the large difference between penetration of Al and Si and the other elements is apparent. It also appears that the scrubber is a source of sulfur-containing aerosols, possibly in part from gas to solid phase reactions. The soluble elements, such as Cu, Zn, and Cr, appear to be generated from the evaporation of the scrubber liquor.

SUMMARY

The use of IXA for aerosol analysis and control device evaluating has been briefly discussed. The advantages of simple preparation and nondestruction of the sample make it attractive for source test samples.

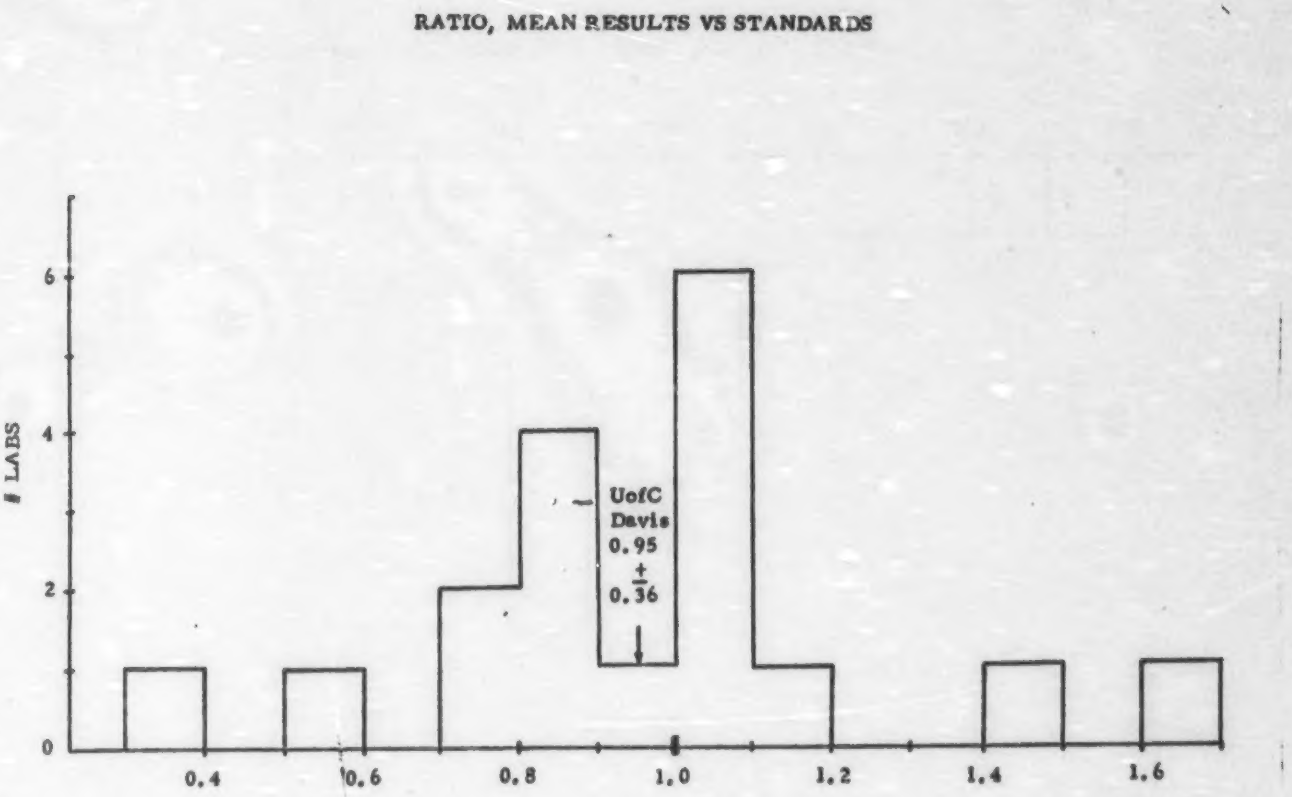

Figure 6. Results of Interlaboratory comparison for ground rock.

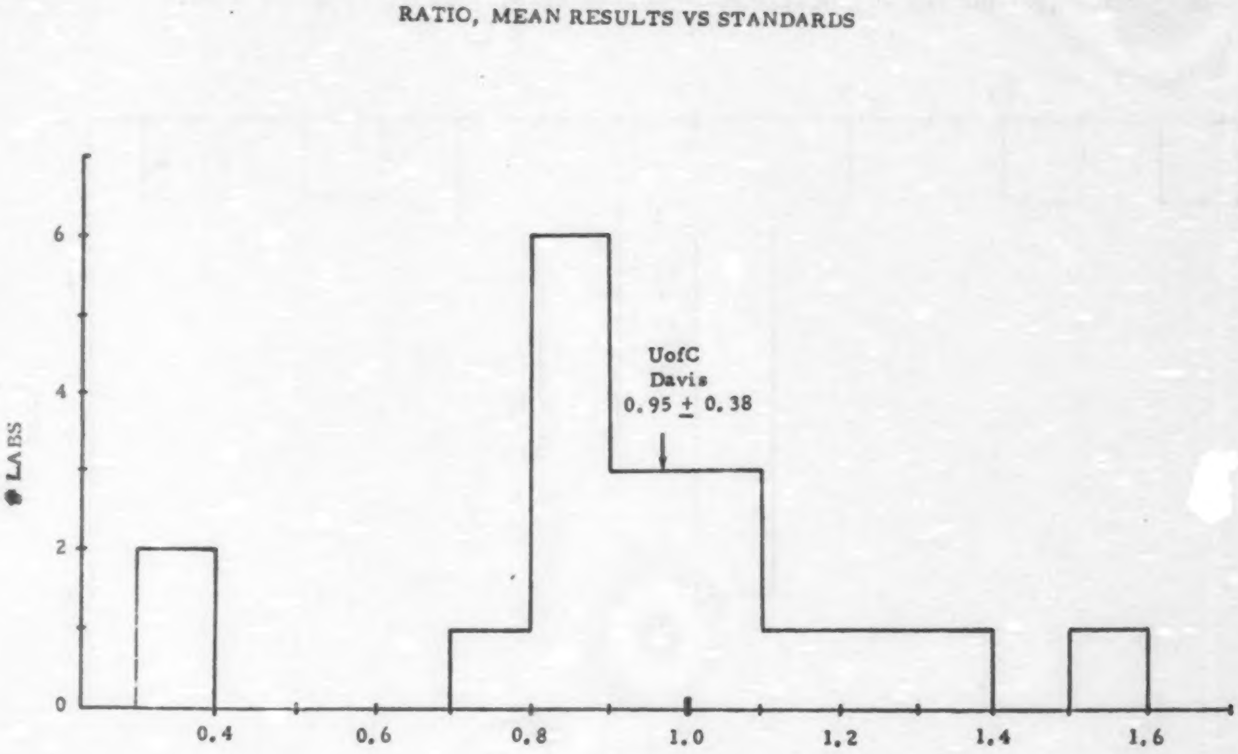

Figure 7. Results of interlaboratory comparison for fly ash.

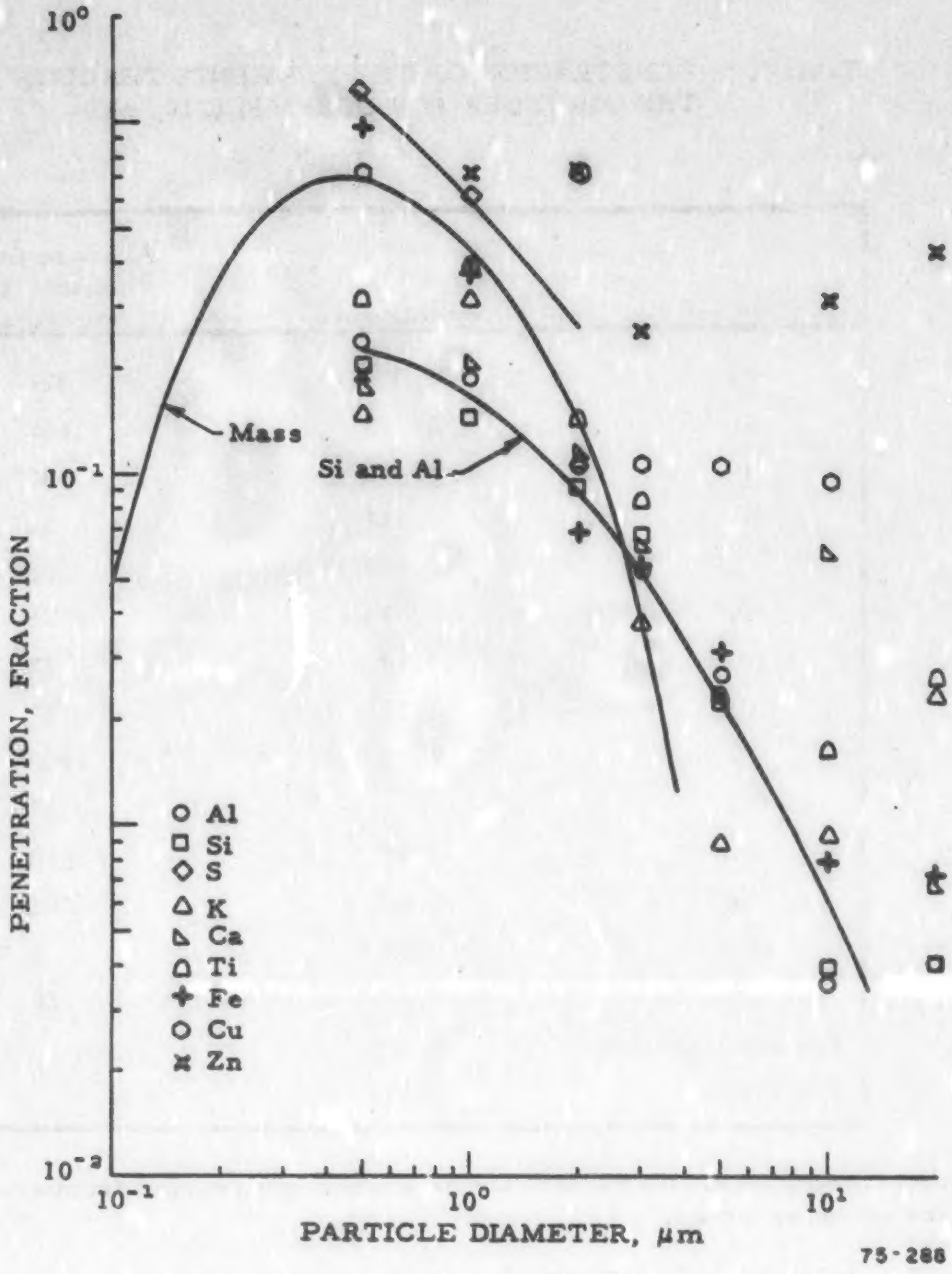

Figure 8. Example of scrubber penetrations for selected elements as particulate matter in the flue gas.

Table 1. PENETRATION OF THE ELEMENTS THROUGH THE SCRUBBER FOR DECEMBER 10, 1974

Element	Penetration	Average Outlet Concentrations Micrograms/DSm^3 [b]
Al	0.029	326
Si	0.033	658
S	3.4 [a]	1030
K	0.043	50
Ca	0.059	508
Ti	0.073	96
V	0.14	27
Cr	1.10 [a]	57
Fe	0.18	1500
Ni	0.95	33
Cu	2.9 [a]	668
Zn	1.5 [a]	501
Br	0.28	5.7
Pb	0.64	120
For All Elements	0.108	
Total Mass	0.074	

[a] Penetrations greater than 1 indicate generation of particles from evaporation of the scrubber liquor.

[b] Dry Standard, 21.1°C, 760 mm Hg

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Ensor, D.S., T.A. Cahill and L.E. Sparks (1975): "Elemental analysis of fly ash from combustion of a low sulfur coal", presented at the 68th Annula Meeting of the Air Pollution Control Assoc., Boston, Mass, June 15-20.

Camp, D.C., A.L. VanLehn, J.R. Rhodes, and A.H. Pradzynski (1975): "Intercomparison of trace element determinations in simulated and real air particulate samples", X-Ray Spectrometry, 4, p. 123.

DISCUSSION

_____: What grease did you use? Apiezon?

ENSOR: Yes. It's probably about the very best thing you can find.

_____: It doesn't give background readings?

ENSOR: No, it's pure hydrocarbon. One intriguing thing was that when we started on this Tom [Cahill] used Apiezon M in some of the screening tests. I later sent up some of the greases that we were actually going to use. Apiezon M apparently has Bentonite in it, and all kinds of trace elements showed up. We had some pretty hurried telephone conversations about what to do with the background. Eventually we used the Apiezon L, which is apparently pure hydrocarbon.

_____: I noticed that copper was high in penetration. Do you think that was interference in the instrument, or something in the background or is it real? I can understand the sulfur.

ENSOR: We found at this source that a lot of the scrubber liquor was being entrained. We were seeing the actual liquor itself.

HARRIS: Thank you.

GENERAL DISCUSSION - WEDNESDAY

HARRIS: Well, that's the formal program for today, and I'm hopeful that we could use the remaining time we have, an hour and a half or so to kind of bounce around some of the problems that we've raised. If there are any problems that you have maybe some of the people can help with of just anything in general that involves this whole problem. Seymour is going to have to cut out, I figured he might be able to go through the rest of his list. We would ask that for this type of thing, if we could have you identify yourself a little bit, so that we can give you credit for or blame for it when we get the transcripts going if there are any questions. Anybody want to lead off?

BOLL: I'm Dick Boll from Babcock & Wilcox. You will recall that sometime earlier Dr. Calvert and I were debating the question of whether or not a temperature drop occurs within each impactor stage due to the pressure drop. He suggested that the temperature drop might cause condensation on the particles, causing them to stick to the target in spite of high jet velocities, and I maintained that no temperature drop is possible because the expansion is isenthalpic, not isentropic. In the meantime, we have put our heads together and come up with a hypothesis that is consistent with both points of view. It is this: Within the flowing gas jet itself, the temperature does, indeed, drop due to isentropic expansion. However, after the gas makes the turn around the target and its velocity is decreased by turbulence and friction, the temperature rises back up again to quite close to the initial value. The temperature of the pile of deposited material on the target is, of course, the initial "stagnation" temperature, since all of the gas within this pile is moving with essentially zero velocity. Thus, since the temperature drop might be substantial in cases of high-velocity jets (of the order of 50° F), condensible materials, e.g. sulfuric acid, might condense upon the particulate and act as a glue to hold the particles together at the instant of impaction. Thereafter, one would expect the "glue" to revaporize, but adhesion once established might be maintained anyhow. Interestingly, this theory seems to explain the Southern Research observation that particle bounce occurs when ammonia additive is on but stops when it is turned off.

MCCAIN: No, it was the other way around.

HARRIS: No. That conforms. You're using it as a surface conditioner when it's on--it's just like you're using the acid as a surface conditioner when it's condensing in the jet--and once it's deposited on there, it doesn't really matter what goes on.

BOLL: All theories should have a converse, which is also true.

HARRIS: Well, we got a theory out on the table anyway. Any comments or anything else they want to go over. Problems?

_____: I was ju't wondering about the problem of trying to get efficiency data on control equipment and the question of agglomeration influencing the efficiency data, what people have done in terms of looking in to see that fraction coming out. Is it what we thought it was going in, or defining that it's not, looking at pictures of it and that sort of thing. Has there been much done in that direction?

HARRIS: Looking at the catch on the individual stages and seeing if it's an agglomerate or if its still wet coming out. I, as a personal comment, I would tend to think that you would have an awful lot of trouble trying to look at a catch of an impactor stage and determine which one of those particles, other than to assume that the inertial separations that we are operating these things under are separating out the particles in a reasonable manner. I was a little concerned with Lee's use of Coulter analysis for analyzing the stage efficiency or the stage sizing, on those things to because most of the Coulter Counter techniques rely on getting a very good dispersion of particles. In fact, it's such a good dispersion that breaks everything up that's agglomerated.

BYERS: Let me relieve your concern. I've never done that thing in my work, there are people in other parts of the company who requested me to give them data from my impactors, and they used the Coulter Counter for their own purposes. But, I would agree with you. There is no way to reconstitute the particles in the distribution which existed in the gas phase, once you have them in the liquid phase. That's what the Coulter Counter requires, so I wouldn't put any confidence in a Coulter Counter distribution.

HARRIS: At least not the kind which analyzes the separate stage.

BYERS: To answer your question, though, one way that we could do it is to sample on a membrane filter, on the inlet and outlet of the control device. Then just look for agglomeration if that's a problem that you're worried about.

HARRIS: Membranes are going to give you a problem because you're not going to catch anything that's less than the particle size in the pore size of the membrane filter.

BYERS: A 0.2 micron pore size just about catches everything.

MCCAIN: If you're getting many one or two micron particles, almost nothing but 1 and 2's, in large quantities on a stage that's not supposed to collect anything smaller than about 10 or 15 microns, you've got a pretty good indication that you've got substantial agglomeration.

_____: You've seen something like that?

MCCAIN: Yes. At some precipitators we've seen that to be the case. Some people from Lawrence Livermore Laboratories (I think they used one of your impactors, Mike [Pilat]) have found the same thing. They attributed it at that time, in a preprint I read, to finite collection efficiency for all particle sizes on all impactor stages. They concluded that it (the impactor) didn't work. That is, it was actually collecting one micron primary particles on stage zero or stage one, the first stage.

_____: We've done considerable work, both optical and scanning electron microscopy on collection plates, and you can, for certain types of emission sources, characterize the particles as long as you don't overload the stage. Even those that are overloaded, with significant mounds, some types you can see all blended together. I think that a lot of it depends on your coating techniques to put on a conductive surface. If you put too thick a layer of conducting material, it masks the individual particles.

HARRIS: One thing that wasn't mentioned was that some of the work you have done with the electrostatic effect, just on the effect of the collection material itself, influencing the electrostatics, not so much as whether you are neutralizing ahead of time but whether you had a conducting or non-conducting substrate that you were putting on. If I remember right, the conducting substrate gave you more problems than you would with the non-conducting substrate like fiberglass. Due to electrostatic effects.

MCCAIN: I have a slide that shows that. There can be some electrostatic effects. I would hate to draw that one from memory.

_____: This is data on insulated surface versus a grounded surface.

MCCAIN: Yes, let me find the right one. (Figure 1). Pardon the way this is presented. If I were doing it over I think I'd do it the other way. We took the ratio of concentration observed with the neutralizer to the concentration observed without the neutralizer instead of the other way around. I would have been, philosophically, a little better to put it on the other basis. This was done with an optical particle counter using a Brink impactor in two or three configurations, one of them was just a simple probe. First what happens with only the probe with and without a charge neutralizer at a fairly low flow rate, 0.1 CFM. Very little effect about 2 microns, a substantial effect a 1 μm and then back down. This all was done at the exit of the pilot scale electrostatic precipitator, operating at moderately high efficiency. The (red) diamonds are what happens to the Brink impactor under the same circumstances showing relative exit concentrations from stage 2 using metal substrates. For some sizes, at that flow rate, there is a trememdous change without the neutralizer apparently due to electrostatic effects in the impactor. This is glass fiber substrates, and you can't see much difference. Now why glass fibers would do what they did toward reducing the electrostatic effects, is somewhat uncertain.

_____: That's just greased metal?

MCCAIN: That's just bare metal, grounded. Just a stainless steel plate. Now, the same system and for the probe and impactor, just to see whether or not the data was real. (Figure 2) That is, was what we were getting with the particle counter real? We took the same system (probe and impactor) and measured it with the particle counter and then did it again gravimetrically. Figure 2 shows the comparitive results. One curve shows the predicted results based on the probe and impactor data using the particle counter. The particle

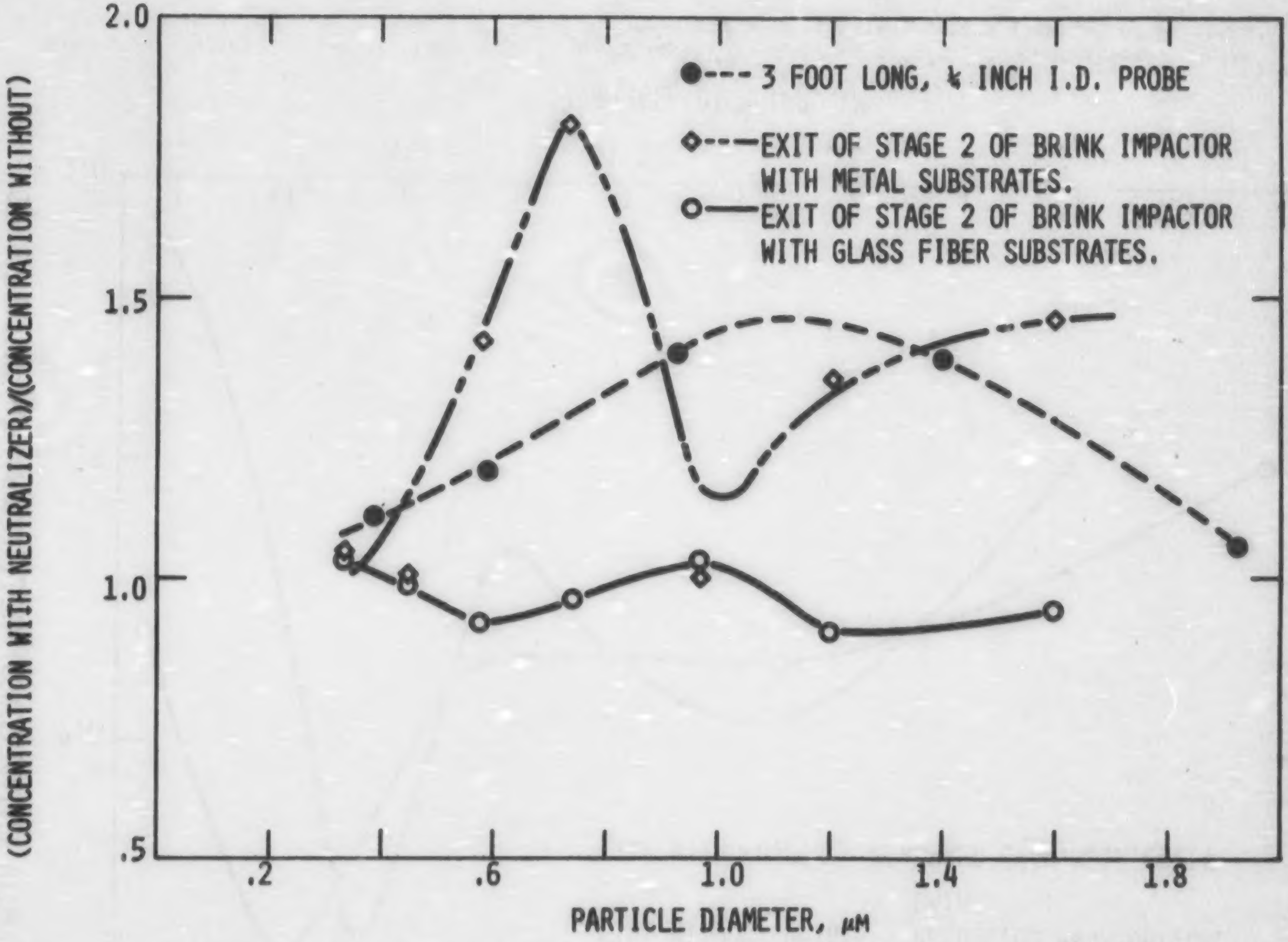

Figure 1. Electrostatic effects in probe and partial blank impactor. All flow rates 0.1 cfm.

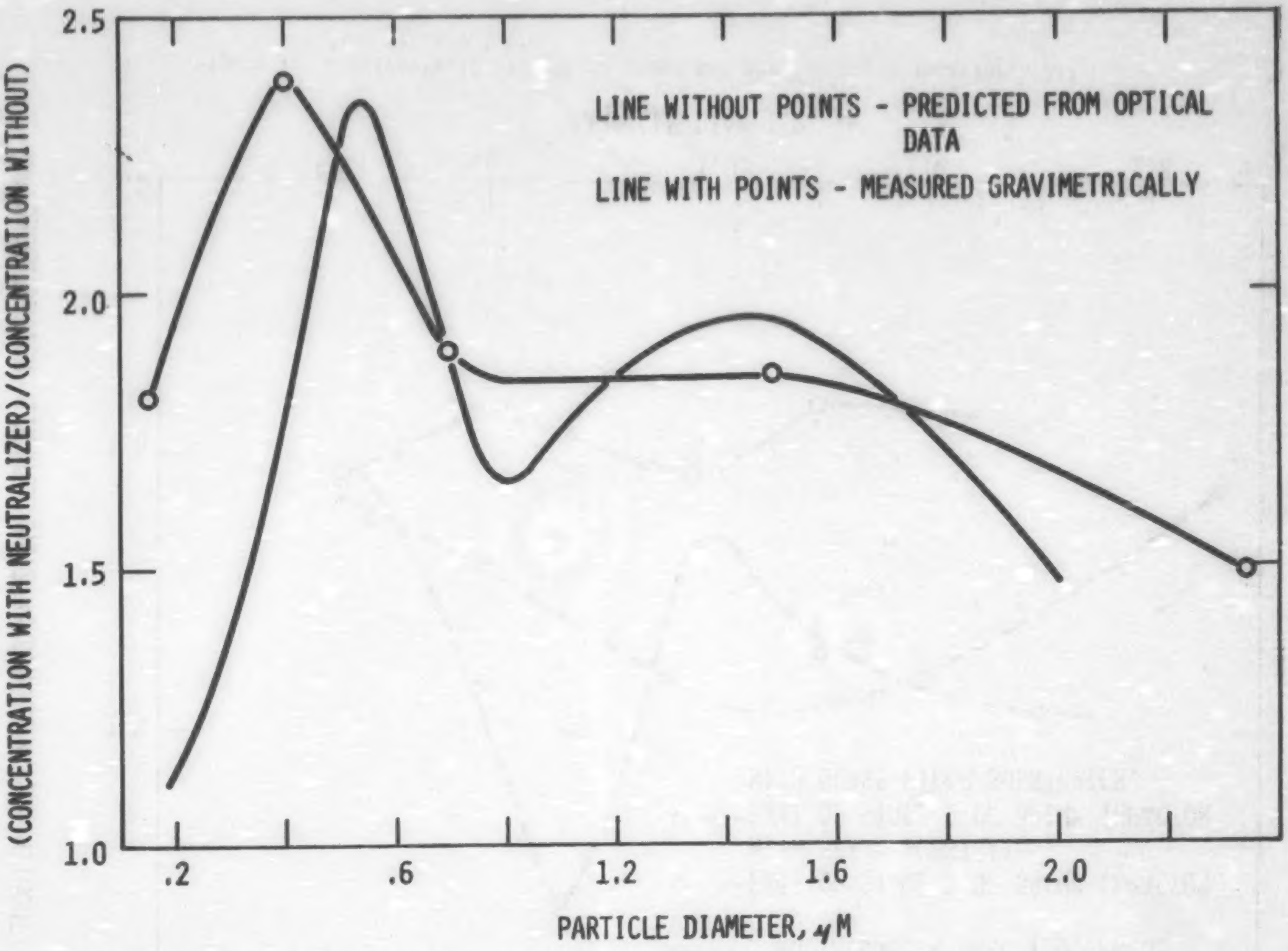

Figure 2. Typical effect of electrostatic probe losses on observed particle concentrations. Brink impactor with 3 foot, 1/4 inch I.D. probe and metal substrates at a flowrate of 0.1 cfm.

counter running out of detection ability at about 0.3 μm. The second curve shows what we measured gravimetrically and was pretty much what we had predicted from the particle counter data. So it would appear that electrostatic effects can be important, at least with bare metal substrates. The effect was a factor of about 2½. That is, the concentration as observed with the neutralizer was 2½ times what it was without the neutralizer, at the exit of stage 2 of the impactor.

SMITH: Do you remember what the D_{50} for that stage was?

MCCAIN: I can't recall at the flow rate. It's probably, at that flow rate, about 2.5 or 3 microns. I don't really know.

_____: Does this mean that electrostatically charged particles are collected at higher or lower efficiency?

MCCAIN: With the neutralizer we measured a high concentration downstream of the stage so it was collecting particles; these particles are definitely too small to be collected on that stage by impaction. They are below the cut point, for that stage operating at that flow rate. So they should have been penetrating. We were collecting them by electrostatic deposition.

HARRIS: Essentially the electrostatic was enhancing the collection more than it should.

MCCAIN: As one might expect.

RAO: The experiments I have done show the same effect. If you don't neutralize the charge on the particles you have higher collection. Some of the experiments of Dr. Lipkin at New York University Medical Center on cyclones show the same thing. If you have charge on the particles and you don't neutralize them, the curve is shifted to the left and the slope is lesser.

MCCAIN: The surprising thing was the fact that glass fiber substrates didn't show any effect at all. It didn't seem to make any difference, which is very satisfying when we were using glass fiber substrates in stacks downstream of precipitators for which we have no satisfactory charge neutralizer to put in the stack. Whether a grease layer may do the same thing as glass fibers, we don't have any data.

_____: You would tend to think that, because it was nonconductive, the glass fiber would allow an electrical charge to build up on it and it would tend to repel the incoming particulate, opposing the inertial collection.

MCCAIN: In another size which we didn't measure, there may have been an effect like that.

_____: Where it would enhance collection on the conductive surface.

SPARKS: Joe, on those tests you ran at the pulp mill with bare metal substrates, did you use charge neutralization?

MCCAIN: No. We don't have anything we can put in that will stand the flue conditions.

SPARKS: Then why do you use bare metal substrates?

MCCAIN: It sticks, the material sticks very well (so greases are not needed for particle retention). We also ran, just for your satisfaction, Les, both bare metal substrates in the University of Washington impactors and glass fiber substrates in Andersen impactors. We'll see what the results look like. They are not analyzed yet, but we did both and, hopefully, they'll both look the same.

SPARKS: You mentioned, why would you expect them to look the same if this is the behavior you see with and without charge neutralizat.on.

SMITH: This is very limited; this is an afternoon's work that you're seeing here.

MCCAIN: We were looking to see if there was potential effect. Yes, there is a potential effect.

HARRIS: Would you expect with a high moisture content that you find in a pulp situation that you're going to have the electrostatic effect?

MCCAIN: We don't have enough information. We don't know.

SMITH: To our knowledge there is not a suitable source available in the temperature range, Seymour has looked into that and I believe he said he gave up, right? Temporarily, anyway.

CALVERT: Temporarily; we're still at it but it gets complicated if the temperature goes very high.

HARRIS: It's kind of hard to carry your own nuclear reactor around with you.

CALVERT: Yes, the licensing problem gets sticky. They keep taking us round and round on that.

_____: Joe, what was your material on that?

MCCAIN: The aerosol? It was DOP.

_____: The unneutralized particles that you were catching, were they being collected where you expected them to be or were some of them on the wall or what?

MCCAIN: We did not look to see where they were winding up. They shouldn't have been collected at that stage, particularly here. This size should not have been going out on that stage and (they) were without the neutralizer. We didn't look further downstream; we stopped and used that part of the impactor with the particle counter.

_____: Yes, but what you're saying, you're putting a space charge into something that is grounded with, you know, some of your smaller particle sizes, .4 of a micron, may be diffused to the wall.

MCCAIN: Oh, you mean where they went out?

_____: I'm curious where the particles that didn't get through are being collected.

MCCAIN: Keep in mind that this was one afternoon.

_____: The electrical velocity should be low in comparison with the gas velocity, where it should really just influence it right at the impaction point. This is my opinion.

HARRIS: Yes, one thing, of course, this is with the Brink and the Brink does have probably more of an eddy problem than some of the others do, just because in the design of those jets there is a lot of free space in behind them, they can fill up with eddies that you could have some diffusional losses with relatively low air velocities.

_____: And those particles close to that surface would tend to be collected?

HARRIS: Yes, when you clean up a Brink impactor one of the things that you do is that you clean up that cone because it tends to get coated. Seymour, do you have anything? We didn't get a chance to get through all of your problems, do you have any you want to kick around?

CALVERT: Nothing that I have to get unburdened. I think that what we found with a charge neutralizer was that the size distribution measured without the charge neutralizer was bigger than measured with the charge neutralizer. Is that what you found?

MCCAIN: That would be the trend of this; you would be taking out small particles on stages ahead of the stage in which they should be deposited.

_____: Taking out particles you shouldn't be?

MCCAIN: Right, we were collecting, then, too soon in the Brink.

_____: What temperatures can you use the charge neutralizers to, then. You were talking about sampling at 600° at one time this morning.

MCCAIN: Yes, what we were using when we used charge neutralizers was polonium. It won't really go above ambient--you wouldn't want to put it in the stack at all. Krypton in a sealed source would probably take stack temperatures, the problem is getting the licensing - getting a configuration that is suitable and having it accepted by the AEC and getting all the required AEC and State licenses.

HARRIS: Somebody looked at nickel and the possibility of using it.

MCCAIN: Nickel, though, is an alpha source and of very low energy, and the slightest little dust layer on the source and it's going to be ineffective. You've got to have something with reasonable energy to put it in the stack, where you're almost certain to build up at least a monolayer of dust on it. Unless you've got a reasonable energy, the ions won't get out.

HARRIS: One thing, I have brought over some copies of the reports that I alluded to earlier. It's there if anybody wants to pick it up. There are some extra copies here of the last two of the reports that Southern put out. There are a few others here that you are welcome to take the number off of and see if you can get the information, I'm out of other copies.

SMITH: One problem that seems to me to be really horrible and I guess Seymour could, it belongs to him, is sampling wet sources. I guess Seymour and Les maybe could comment on where that stands. I would hate to have to sample the outlet of a scrubber or the inlet if you can't really actually measure what's going in and what's coming out. Some of it's being created in the scrubber and some of it is being evaporated. Where does that technology stand?

CALVERT: We use, generally we use a precutter that has a cut diameter, depending on the sampling rate, of around 8 microns aerodynamic size. Maybe a couple more, maybe a couple less. We essentially, then, throw away the top part of the particle size distribution, say that we're interested in particles smaller than 2 to 3 microns aerodynamic diameter. Generally the penetration for high-efficiency scrubbers has dropped off on to practically nothing at let's say 2 and a half to 3 microns. So that we're saved by that fact, we can cut off the high end of the size distribution and not influence the computations that we're going to be making later.

We've run into, as I mentioned, some situations where the sampler was literally deluged. From what the fellows told me there was just water sloshing all over the place and there was just no escaping it, so, essentially we put a cylindrical shroud out as, it was either a beer can or a welding rod can, sort of prosaic. Sort of this way and then with holes punched in the bottom so that there tended to be an induced gas flow through it and out through the downstream side and then the impactor sticking into the side of it. And we just had to forget about being isokinetic, having the probe pointed in the right direction. There was just no other way to do it. We did some computations about what the bias would be at a particle size of 3 or 4 microns. We concluded that it wouldn't be too much. You have to put up with that error.

SMITH: How about evaporating particles that had nucleated water?

CALVERT: We have a lot of trouble with filters loading up. One of the mysteries is why water will get out to the filter and cause a very high pressure drop on the final filter and not appear on any of the stages in the impactor. We have this happen. But it happens very frequently, so when we get a bad one that way and if we don't want to heat the impactor, then we have to use a separate filter and heat the final filter, keep it dry.

MCCAIN: Have you tried glass-wool plugs?

CALVERT: No.

HARRIS: Why don't you want to heat the impactor?

CALVERT: Because we don't. For instance, if we're in a situation where we don't want to heat the impactor, we want to get the wet size or one we did recently, we were concerned with coke oven emission, we were concerned that a significant part, fraction of the particles, might be organic matter that was volatile. We just plain didn't know in advance. So here we are with hot gas going into the scrubber and cold, say 135° F gas coming out of the scrubber, saturated. So we wanted to have the inlet sample the same temperature as the outlet sample and what we did was pull our samples out of stack, and pull the sample through a probe to the imapctor and have enough cooling in there so that the impactor temperature at the inlet was approximately the same as the impactor temperature on the outlet.

HARRIS: How do you ascribe your efficiencies, what you're actually sizing on the outlet is not the particles, not primary particles, but the growth particle, it's the water. How can you then?

_____: Do you weigh your filters wet?

CALVERT: Well, if we are, all right, that's another problem and if we want to do that then we, what we have to do is determine a wet particle size. Depends on how the scrubber is built, if it has a presaturater on it, quencher, something of that sort and then the gas goes into the scrubber and we want to separate the efficiency of the scrubber from the efficiency of the presaturater.

We'll sample the gas out of the presaturater and at the scrubber inlet cold so that we get wet particle size, and we'll also get wet particle size on the outlet. If there has been additional particle growth in the scrubber due to condensation, we're stuck, and we run into these too, and there we back off and do the same thing and measure the size in and the size out dry and heat the impactors on both sizes. But we're left with then, the necessity to rationalize what we find by some type of theoretical model.

HARRIS: Is the heating of the impactor with the precutter on it to knock out the large rain clouds? Is that by itself sufficient to evaporate these smaller than 5 micron particles or do you have to put some kind of section where you specifically add energy into that thing to evaporate before it gets into the impactor?

CALVERT: We've done it both ways. There have been some cases where apparently we weren't getting enough heat transfer through the impactors so we put a long section of, perhaps, something like 1-inch- diameter tubing between the precutter and the impactor and put a heater on that and then also heated the impactor.

BYERS: Does the Brink sampler that's designed to hold deep dish cups below the plates help with the problem? I think that it was designed to sample liquid droplets.

HARRIS: The biggest problem I guess is whether you're trying to size the droplets or whether you're trying to size the particles.

ENSOR: A philosophical problem more than ...

HARRIS: Well, in your case, if you're going after a model, if it's a dry inlet scrubber, then you've got to size both, as Seymour said, on the inlet and outlet. If it's a wet presaturated scrubber, then you have to go wet to evaluate the droplets.

CALVERT: The problem isn't with retaining the drops on the stages but, and, as a matter of fact, if we have that kind of situation we're more likely to use glass fiber paper substrates, and they will blot up the liquid. But the problem comes in in the liquid getting through to the final filter and decreasing its porosity or permeability and giving very high pressure drops, so it will cause the, well, the sampling period to be cut as short as 30 seconds. We've had that happen. There's still a little bit of sample in there.

BYERS: Could you separate that final filter out of the impactor and heat it separately?

CALVERT: Yes, that's what we do in that kind of situation. The big problem, the part of my notes that I didn't get to, was facing the fact that you didn't get a clean answer. There are some problems that you just can't resolve and this is one of them. We've gone round and round on this dilemma--should we measure the particle wet or dry--and there is just no perfectly correct experimental way that will give you the information you need to validate the mathematical model without some assumptions, some hypothesis tied in.

HARRIS: The guys from GCA will be here tomorrow; we'll see if they have horror stores about what goes on in baghouses that the scrubber and ESP guys got today.

CALVERT: This is what's called: facing the fact that you can't compensate for everything, at least not in a reasonable way; and trying to get a dry particle sample from a urea scrubber outlet; trying to get as-is particle size distribution for sulfuric acid mist; trying to get impactor conditions that are exactly the same as in-stack conditions. We're hoping that the size selectivity of the particle control instrument will not cause the inlet and outlet characteristics to have different composition and/or characteristics. It can be important if particle growth is important in your mathematical model for scrubber penetration. Or not knowing, again, whether hydration or dehydration changes the morpholoty and/or subsequent susceptibility to wetting.

Error--you just can't have everything in an absolutely unequivocal way.

NEW TECHNIQUES FOR PARTICLE SIZE MEASUREMENTS

William B. Kuykendal, Environmental Protection Agency, IERL/RTP*

SPARKS: The first speaker today is Bill Kuykendal of the Process Measurements Branch and he is going to tell us all about weird and wonderful things that are going to solve all of our problems.

KUYKENDAL: I am in a group of process engineers who are interested in developing control techniques for pollutants and emissions from stationary sources. We are, therefore, interested in being able to evaluate the fractional efficiency of particulate control devices and, hence, our interest in particle sizing techniques.

Because of this interest, we undertook a study to evaluate several different devices developed by EPA and others to measure particle size distributions. The intent in our study was to determine how the instruments compared with one another rather than an absolute determination of accuracy for each device. The devices and instruments which will be described are listed in Table 1.

Figure 1 shows the Southern Research Institute series cyclone used in-stack. In addition to size distribution information, our group feels that determination of chemical composition as a function of size is desirable. The use of a set of cyclones offers several advantages in this regard over impactors. First, and of prime importance, a large amount of sample can be collected for analysis. Also, since substrates are not used, the problems discussed in earlier papers are not encountered. Three cyclones are used which gives four size fractions, one for each cyclone plus one for the back-up filter.

Figure 2 shows the PILLS-IV by Environmental Systems Corporation. It is an in-stack instrument with a purge box, a probe containing a gallium-

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*Coauthored by Charles H. Gooding, Research Triangle Institute

TABLE 1. INSTRUMENTS

Instrument	Company	Status
Brink Impactor	Monsanto Envirochem Inc. St. Louis, Mo.	Commercially Available
Andersen Impactor	Andersen 2000 Atlanta, Ga.	Commercially Available
Series Cyclone	Southern Research Institute Birmingham, Ala.	R&D
PILLS IV	Environmental Systems Corp. Knoxville, Tenn.	Commercially Available
In Stack Beta Impactor	GCA Corp. Bedford, Mass.	R&D
Piezoelectric Microbalance Impactor	Celesco, Inc. Irvine, Ca.	Commercially Available

arsenide laser, the detection optics, and the processing electronics. Figure 3 shows a schematic of the instrument. The control electronics causes the laser to pulse. The detection optics then looks at two angles in the forward scattering mode, ratios these angles, and then puts out the size distribution which is a function of the intensity ratio. That is then output on a paper tape. It takes about 5 minutes to process a sufficient number of particles to compute the size distribution.

Figure 4 shows the IBC-Celesco instrument. It was principally developed for ambient air monitoring. It is a 10-stage cascade impactor that uses piezoelectric microbalance technology for sensing the mass on each of the stages of the impactor. Since this device was developed for ambient air sampling, it could only be operated for a very brief period of time because of the higher dust loading in the wind tunnel. The sample enters a 3-way valve. Purge air (filtered room air) is pulled through the valve during the nonsampling mode. To sample, the valve is flipped over to the sampling

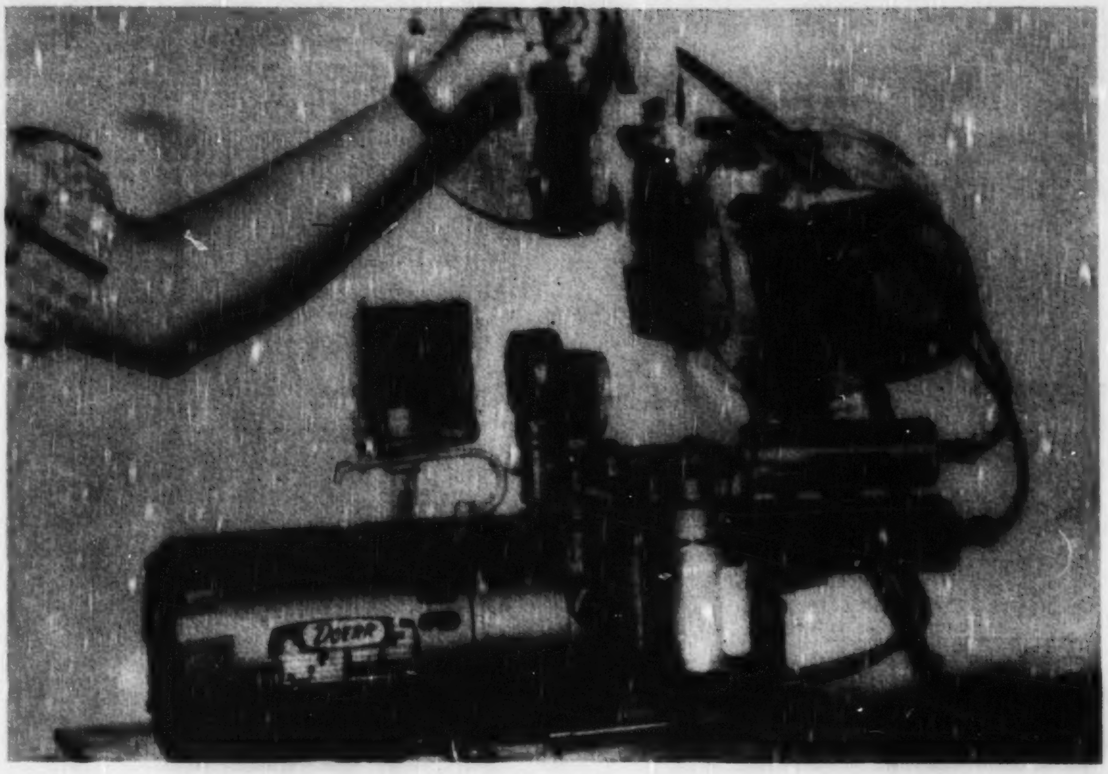


Figure 1. Southern Research Institute series cyclone.

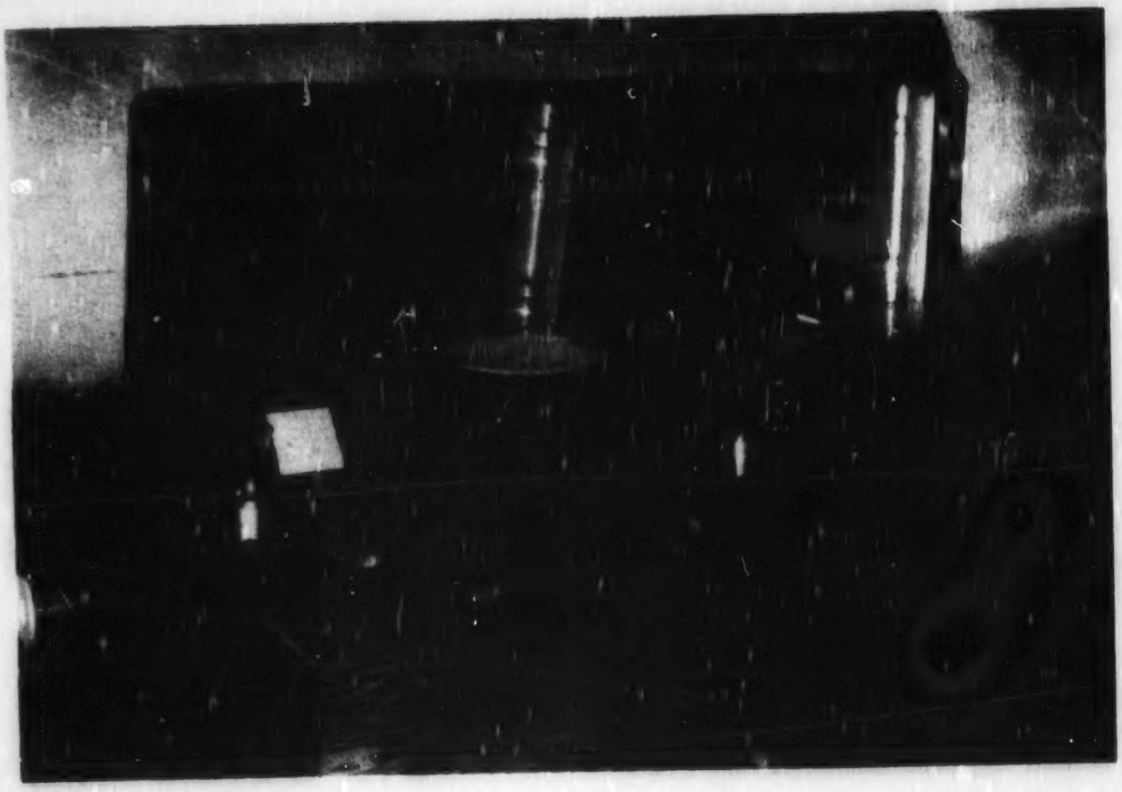


Figure 2. PILLS-IV.

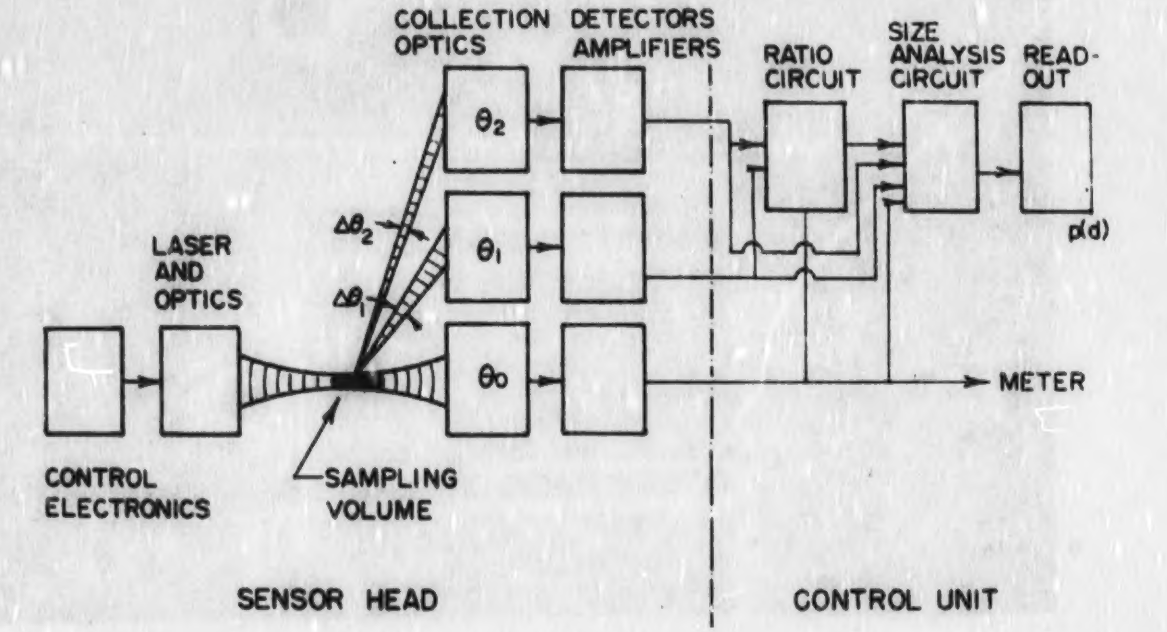

COLLECTION OPTICS
DETECTORS AMPLIFIERS
RATIO CIRCUIT
SIZE ANALYSIS CIRCUIT
READ-OUT
θ_2
θ_1
θ_0
$\Delta\theta_2$
$\Delta\theta_1$
LASER AND OPTICS
p(d)
METER
CONTROL ELECTRONICS
SAMPLING VOLUME
SENSOR HEAD
CONTROL UNIT

Figure 3. PILLS-IV schematic.

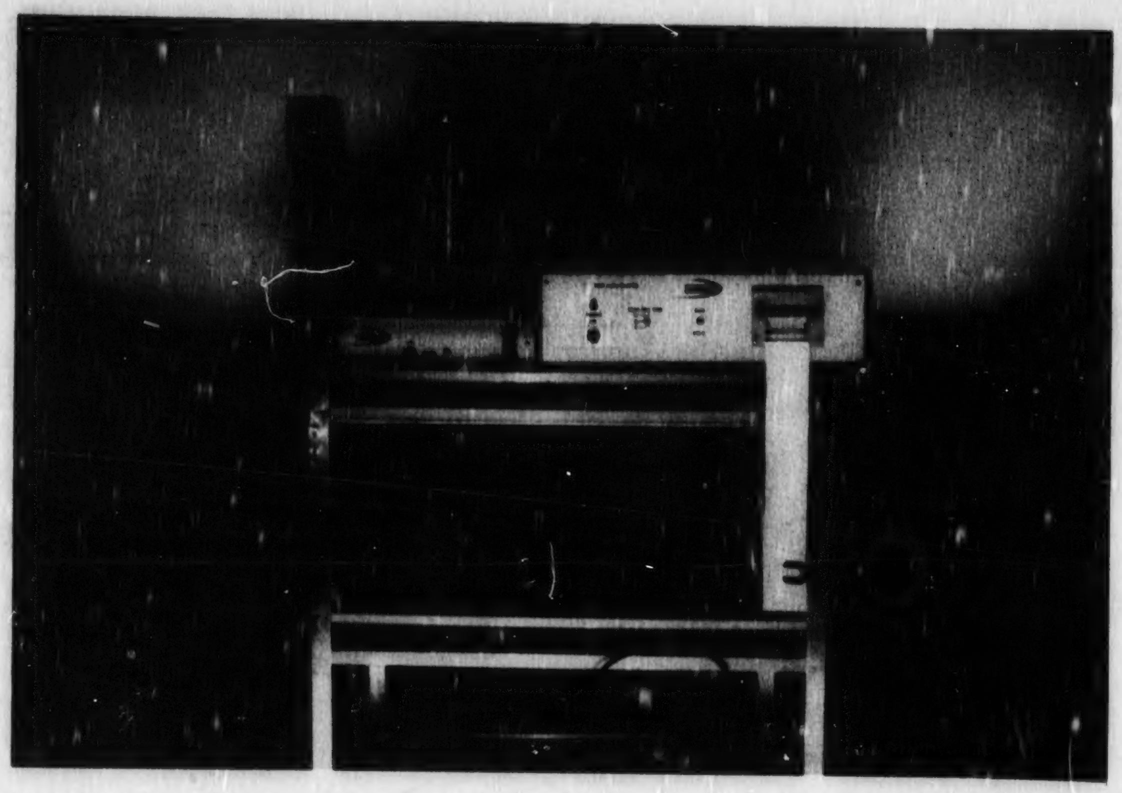


Figure 4. IBC-Celesco piezoelectric microbalance impactor.

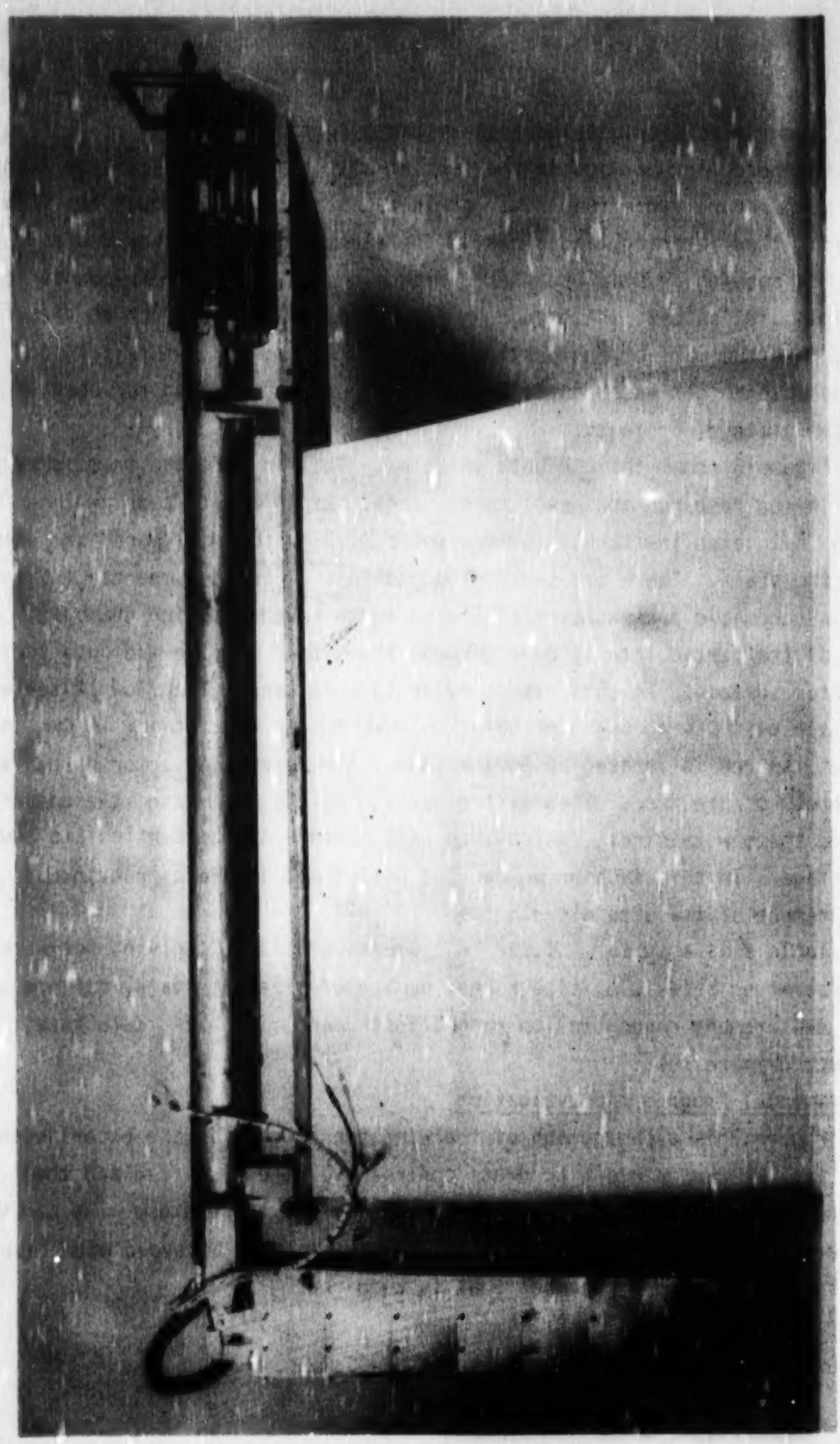
mode, and the particulate-laden gas impacts on the sensing crystal. There is a reference crystal behind the sensing crystal to compensate for changes like temperature variation. The gas then passes on out through the flow control, flow metering, and blower. The electronics ratio the two signals from the sensing crystal and the reference crystal, convert frequency to voltage, and indicate the change in frequency, which then becomes directly related to the mass of the particulate on each stage. In this fashion, we are able to get mass concentration as a function of size for each of the ten sizing increments.

Figure 5 shows the GCA beta impactor. This is a device that is very much in the research and development stage. It is a beta-sensing impactor which is located in-stack. It requires a 20.3-cm (8-inch) port for insertion into the stack. There are seven elements in the cascade impactor. Figure 6 is a schematic presentation of one of these seven sampling chambers. The flow of the particulate is down through the sizing orifice and onto the impactor surface. In this case a mylar tape is used. The flow passes around the tape cassette through the holes on down to the next stage. A carbon-14 beta source is located below the tape. A reference detector gauges the thickness of the tape. Then as the particulate is collected, the detector gauges the new thickness, which then is a measure of the particulate mass per stage. In this fashion we can get real-time, in-stack, continuous measurement of the size distribution.

Table 2 is a summary of the instruments considered, giving for each the operating principle, size range, number of size intervals, the approximate measurement concentration range, instrument flow rate, data rate, and approximate cost.

Experimental Program and Evaluation

Figure 7 is a photograph of the wind tunnel used in the experimental program. We can maintain accurate control over the dust feed and the volume of gas through it, as well as humidity and temperature. We can vary temperature up to 205°C (400°F). The tunnel can also be doped with gases to simulate stack conditions when that is required.



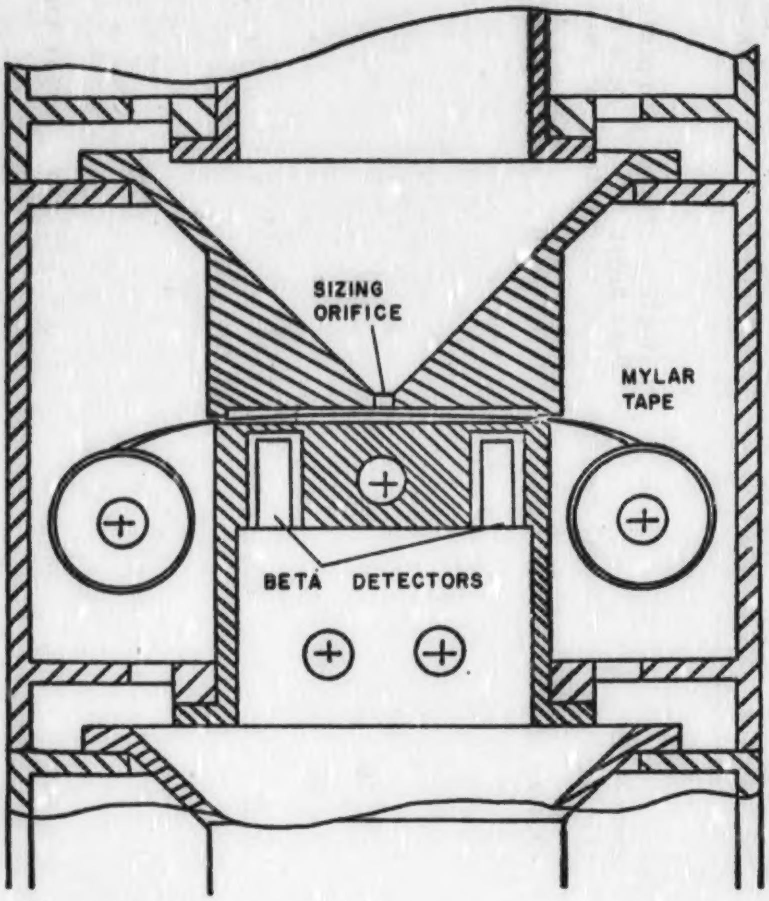

SIZING ORIFICE
MYLAR TAPE
BETA DETECTORS

Figure 6. Schematic of one of the sampling chambers of the GCA beta impactor.

Table 2. INSTRUMENT SPECIFICATIONS

Instrument	Brink Impactor	Andersen Impactor	Southern Series Cyclone	ESC PILLS IV	GCA Beta Impactor	Celesco Piezoelectric Impactor
Operating Principle	Manual Impactor with cyclone	Manual Impactor with cyclone	Manual cyclone	Single Particle Dual angle light scattering	Impactor with beta detection of mass	Impactor with piezoelectric detection of mass
Size Range (micrometers) (unit density)	<0.4 to >14	<0.4 to >8.3	<0.7 to >3.5	0.3 to 3.0	0.3 to >6.5	0.09 to >35
Number of Size intervals	9	10	4	10	7	10
Mass Conc. Range (g/m^3)	0.1 to 6	0.02 to 3	0.1 to 25	10^3 to 10^6*	0.3 to 20	50×10^{-6} to 0.08
Instrument Flowrate (1/min)	1.4	20	20	NA	9	0.2
Data Rate	manual	manual	manual	batch 5-15 min	real time	batch 10-15 min
Approx. Cost	$2.5K	$3K	R&D	$35K	R&D	$15K

*particles/cc

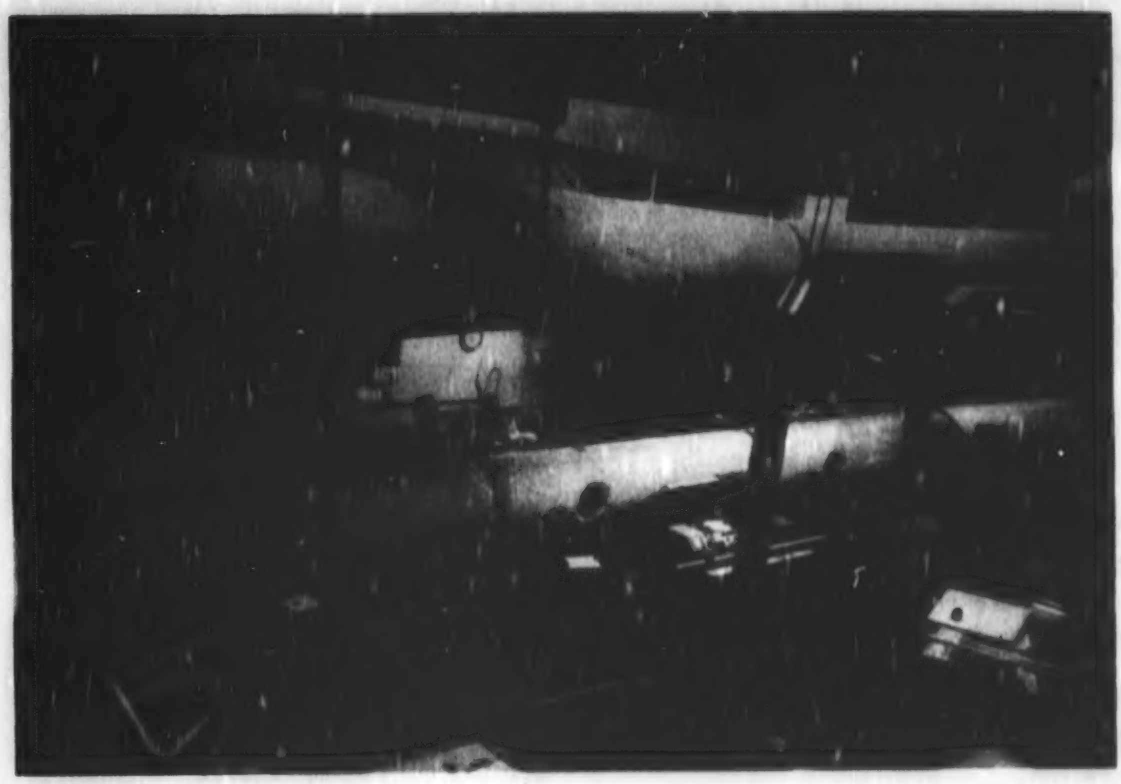


Figure 7. Wind tunnel.

Table 3 shows the wind tunnel conditions as operated. Two particulate concentrations were used: high (0.86 g/m^3) and low (0.08 g/m^3). These were obtained by varying the tunnel air velocity and the dust feed rate. In all cases we were using air at room temperature.

Table 3. WIND TUNNEL CONDITIONS

	High Concentration	Low Concentration
Velocity (m/sec)	9	27
Temperature (°C)	27	27
Relative Humidity	50-55%	50-55%
Gas Composition	Air	Air
Dust Feeder kg/hr	8.3	2.3
Concentration (g/m^3)	0.86	0.08

Since the purpose of this study was to compare the various techniques available, it became necessary to select a baseline method. Because of the considerable experience with the Brink and Andersen impactors, these devices were selected. Figure 8 presents the data taken for reference purposes. The results from the Brink and Andersen impactor runs at the low concentrations have been plotted on a single curve on a log/log scale. There is a certain amount of scatter in the data, but considering that data were obtained with two different instruments, the agreement is good. The curve was generated by a least squares curve fit. At the high concentration, a similar curve was obtained and is shown in Figure 9. The two curves are extraordinarily close in shape. This convinced us that our dust feeder was giving a reproducible size distribution. On each of the remaining plots, the solid line is the least squares curve fit for the Brink and Andersen impactors and the points are the data for the sizing device being evaluated. This allows a direct comparison with the impactor data.

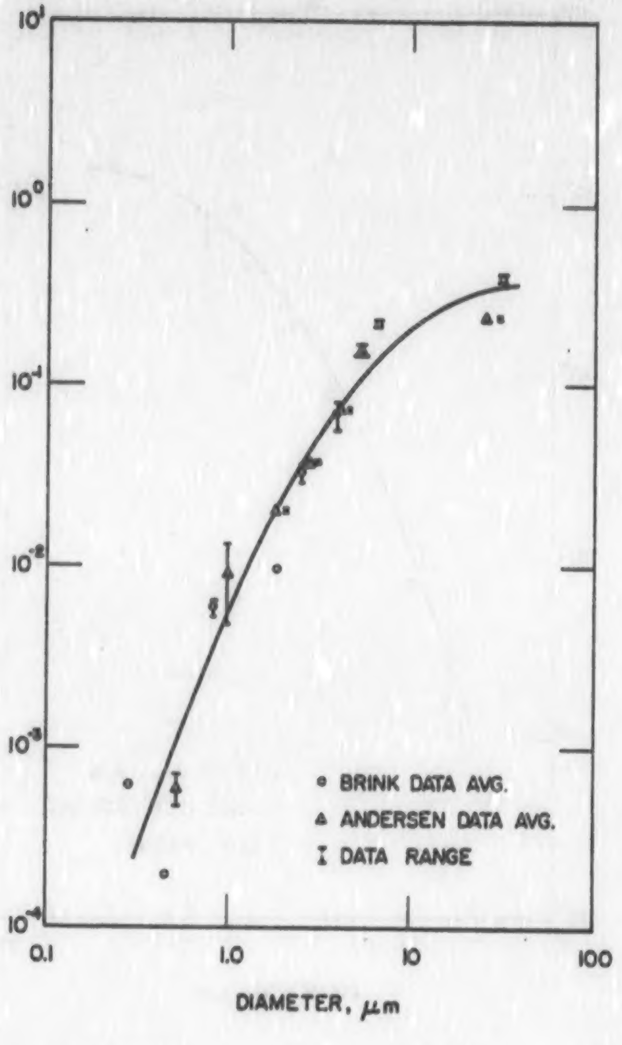


Figure 8. Brink and Andersen impactor data (low concentration).

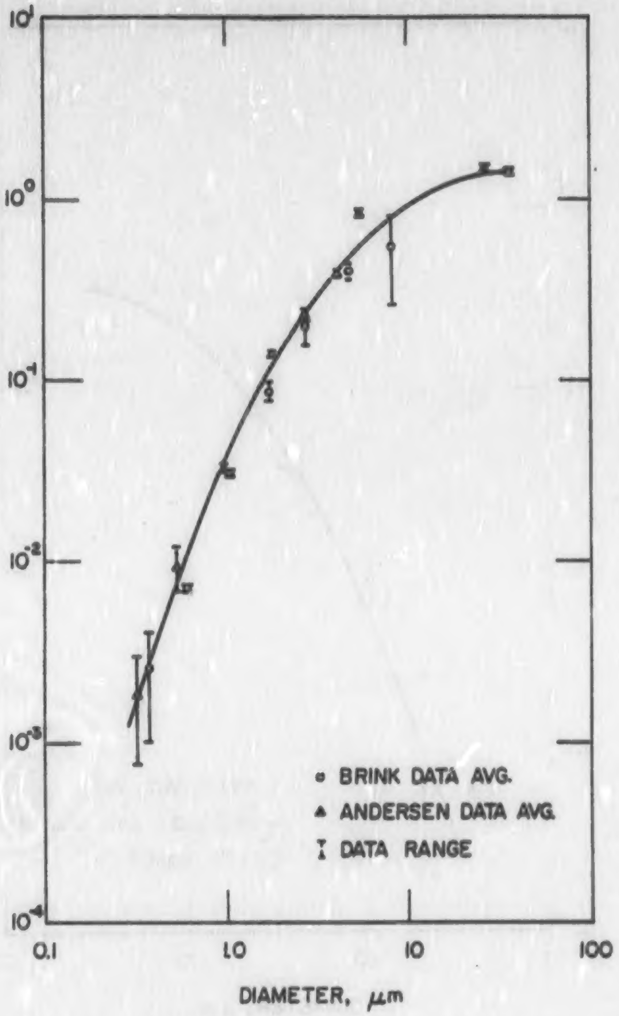


Figure 9. Brink and Andersen impactor data (high concentration).

The data from the Southern Research Institute series cyclone are presented in Figures 10 and 11 for the low and high dust loadings, respectively. The agreement with the Brink and Andersen impactor data is very good. In addition, this device has the advantage that it can collect a large quantity of sample for later analysis without overloading.

Figure 12 gives the information from the IBC-Celesco piezoelectric impactor. Since this instrument was developed for ambient air sampling and was operated in an extractive fashion without benefit of a dilution system, we were only able to test it at the low concentration and then only for a duration of approximately 7 seconds. The instrument has ten sizing intervals. We saw nothing in the first three sizing intervals, and we suspect that this was because of deposition in the probe used to extract the sample. This is borne out in the fourth size interval which reads low with respect to the three subsequent stages indicating that a portion of this particulate is also separated in the probe. Stages five, six, and seven all collected particulate. The shape of the curve is similar to the baseline curve with the values consistently high, indicating a possible calibration error in the Celesco instrument. The last three sizing intervals did not detect any particulate. When the baseline data were used to calculate the expected loading on these stages, it resulted in mass concentrations that were below the detectable limit of the instrument.

Figure 13 gives the low concentration information using the PILLS light scattering instrument. It is obvious that the PILLS does not produce the same type of response as the inertial devices. The data at the high concentration with the PILLS, shown in Figure 14, bear this out Superimposing the PILLS curve at the high concentration on the results at the low concentration shows quite good agreement, indicating that the response of the instrument appears to be

Figures 15 and 16 show data from the GCA in-stack beta instrument. The data seem to follow the pattern of the instruments evaluated earlier.

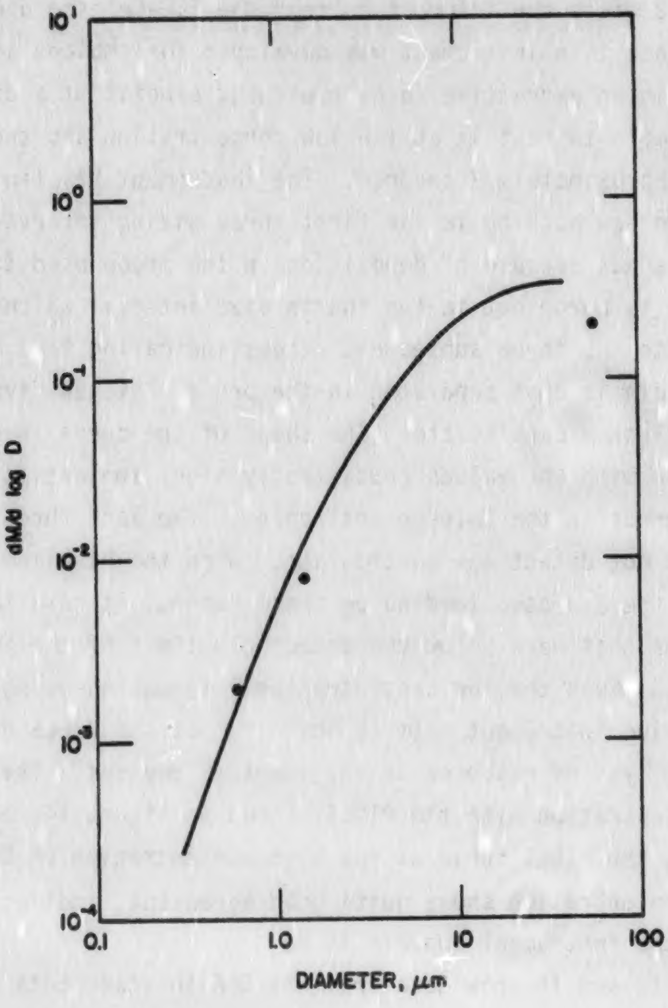


Figure 10. Comparison of Series Cyclone data with impactor data (low concentration).

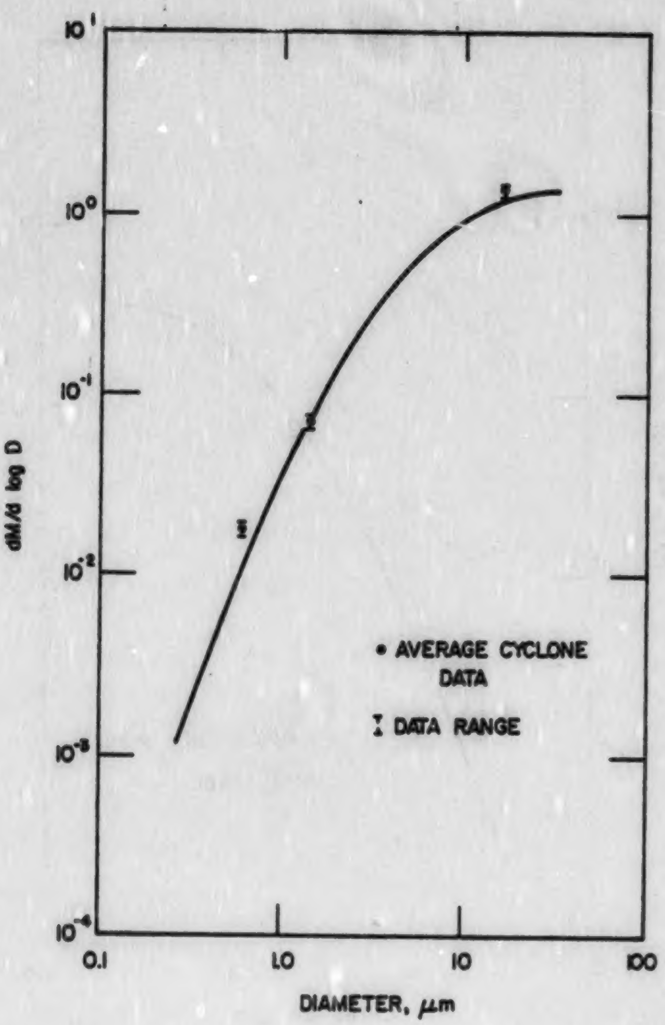


Figure 11. Comparison of Series Cyclone data with impactor data (high concentration).

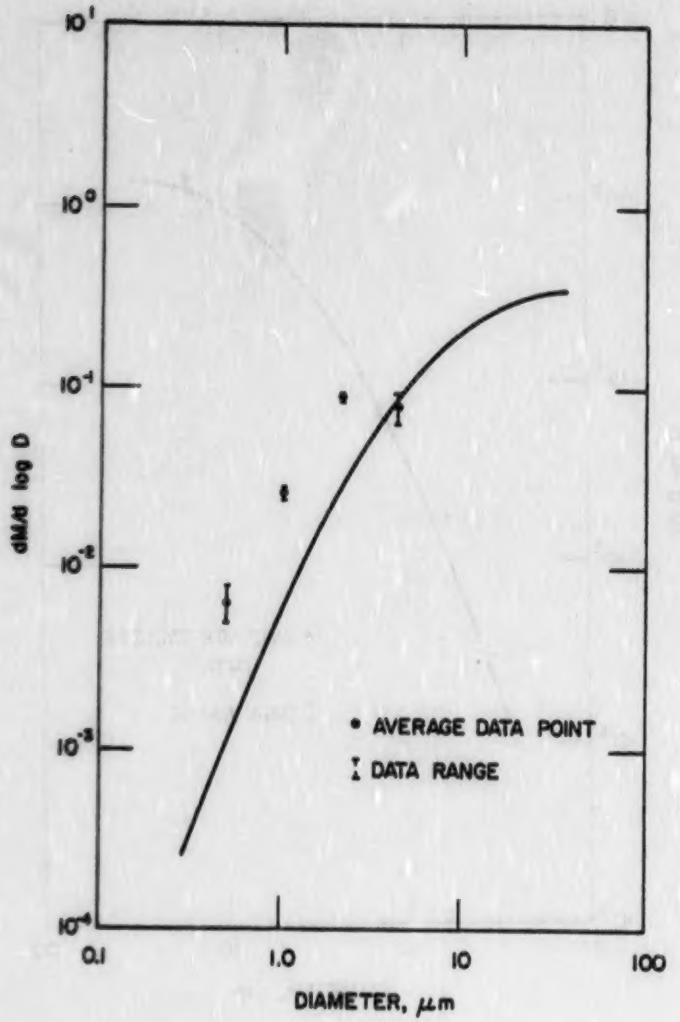


Figure 12. Comparison of piezoelectric microbalance impactor data with impactor (low concentration).

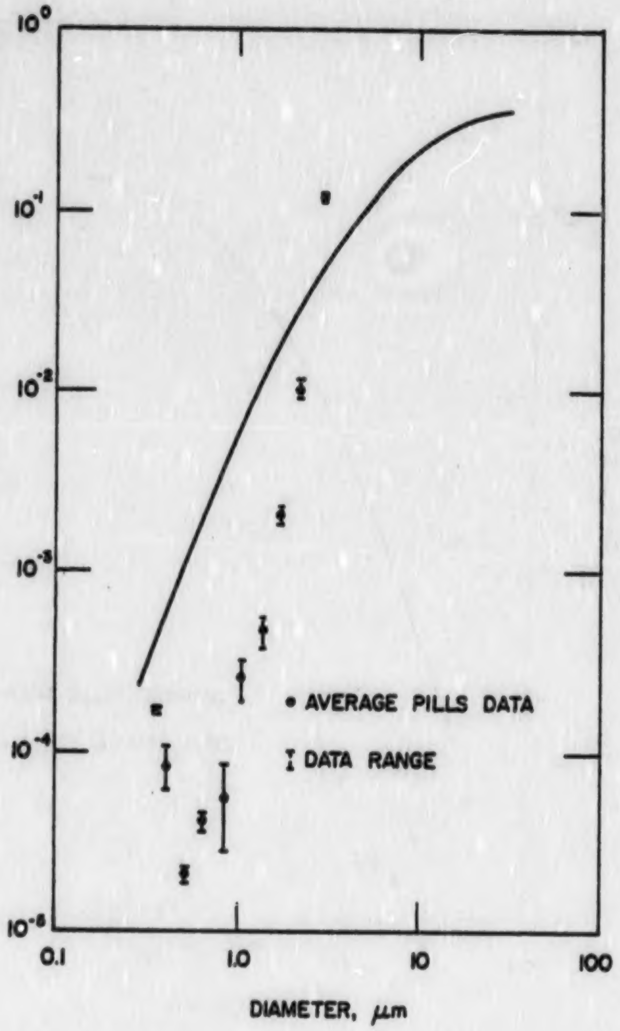


Figure 13. Comparison of PILLS-IV data with impactor data (low concentration).

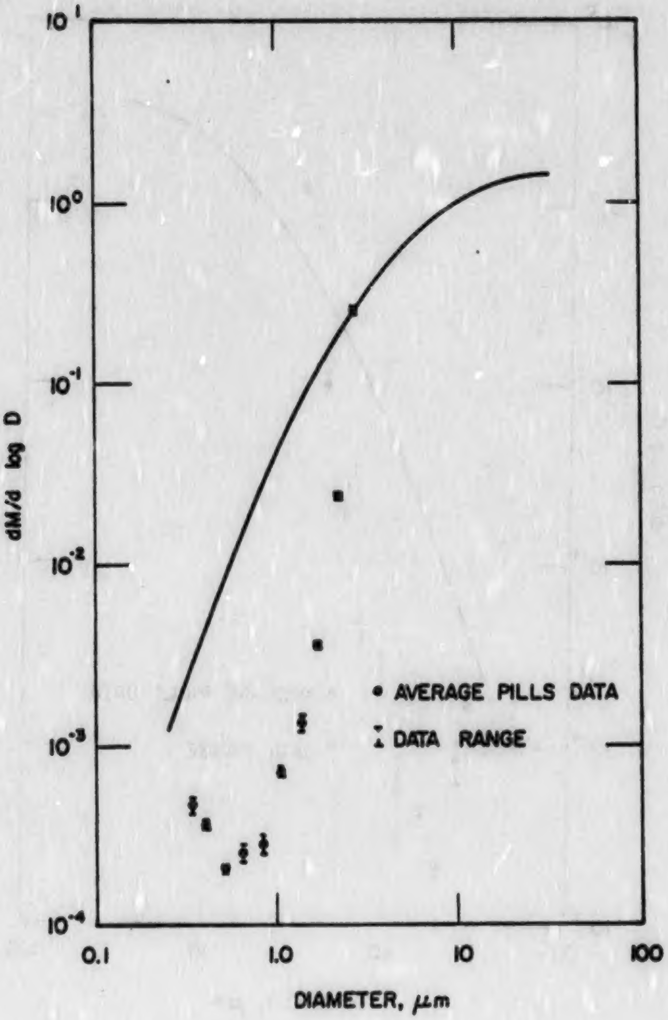


Figure 14. Comparison of PILLS-IV data with Brink impactor data (high concentration).

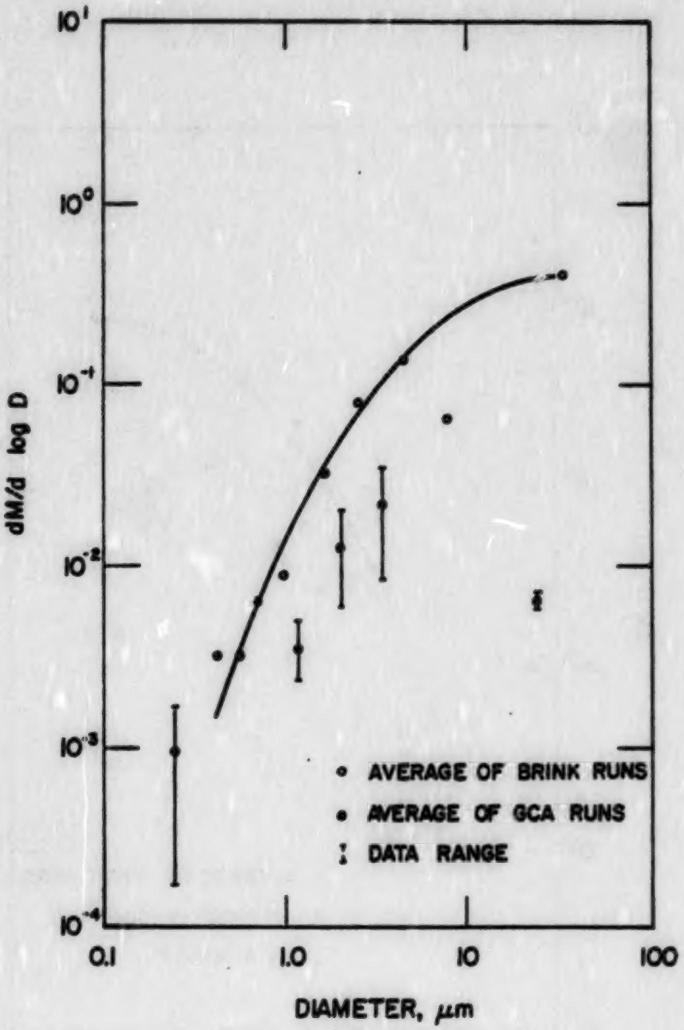


Figure 15. Comparison of in-stack Beta impactor data with Brink impactor data (concentration = 0.24 g/m^3).

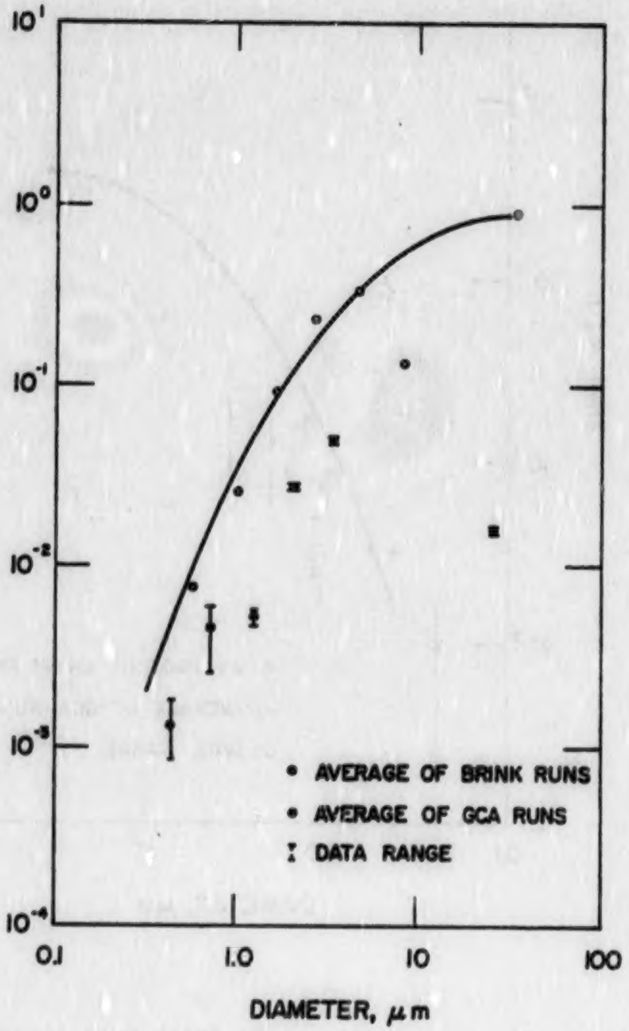


Figure 16. Comparison of in-stack Beta impactor data with Brink impactor data (concentration = 0.86 g/m^3).

Note that the high concentration in these tests is 0.86 g/m^3, and the low concentration is 0.24 g/m^3. The sensitivity of the instrument would not allow us to test it at the earlier conditions. Upon examining the instrument, we found a considerable amount of particulate matter on the wall in the upper stages. This would account, at least in part, for the low readings on the first stage. In the runs at the higher concentrations with this in-stack device, the agreement with the impactor data becomes better.

Summary

I believe we can fairly say that all the instruments are useful in measuring particle size. The devices that employ inertial size separation all seem to agree reasonably well with one another, and therefore can be used in exchanges of data with each other. In the case of the PILLS instrument, which is based on single particle optical light scattering, it would not be recommended that it be used in direct comparisons with data obtained using inertial separators. I should point out that of the two different principles of operation, we are making no distinction as to which is right; one merely responds to a different set of parameters than the other.

DISCUSSION

_____: Series cyclones, were those used in the vertical or horizontal position?

KUYKENDAL: They were used vertically.

BYERS: This PILLS-IV instrument. That is a single particle counter?

KUYKENDAL: It is a single particle counter, that's right. We were relatively sure that we were counting single particles. It uses a gallium-arsenide diode laser, which pulses at 1 kilohertz. We were counting particles only about once every hundred or thousand pulses so we were relatively sure that there were only single particles in the viewing volume.

OLIN: You seem to be using the Brink and the Andersen as a standard. I was wondering if you really felt that to be true. With all the problems we heard about yesterday, I was wondering if maybe you would prefer to believe the PILLS data as opposed to the impactor data.

KUYKENDAL: From a convenience standpoint only, I would prefer to believe the PILLS data to be correct. The PILLS is a much simpler instrument to use. However, the accuracy of the PILLS is subject to question. Keep in mind that our tests were conducted in a wind tunnel at ambient conditions. Therefore, there were no acid gases to react with the substrate. Also, we used coated substrates which we feel reduced the particle reentrainment problem. In general then, we feel that the impactor data accurately characterizes the aerodynamic diameter.

_____: What particular calibration did you use? Did you use Southern's calibration?

KUYKENDAL: We did not make the correction for the smaller sizes.

MCCAIN: Well, our calibration of those impactors would tend to increase the discrepancy between the PILLS and the others. And then there's the question of which one you believe.

SMITH: Those are differential velocities shown; and it turns out that if you make the correction it might not make so much difference as you think, because it would be dividing the DM by a bigger log D; so you tend to just move down that differential curve. It's really not going to just change your size distribution by a tremendous amount.

_____: Even though your D_{50} on the last stages is way less than the calibration that the manufacturers say it is?

SMITH: The error will not be as much as you might think on the differential plot, because you don't move those points horizontally on a differential plot--they'll move over and down.

MCCAIN: We have a little more information on the PILLS too, running at Fluidyne, and there we had other particle counters plus impactors to compare it with. The PILLS showed the same disagreement, it looked almost exactly the same as the data that you obtained.

KUYKENDAL: It seems to be that the PILLS is not corresponding, not conforming to the same particle size as the others. Whether that can be corrected by calibration or not is not clear. It is indicating a change in the right direction; maybe that's sufficient. I don't know.

_____: Has anybody looked at the flow field in that?

KUYKENDAL: That's something we plan to do, but it hasn't been done.

MCCAIN: From the Fluidyne data, where we ran it at two different locations, one with a high velocity gas stream and one with a low velocity gas stream, the disagreement was much worse in the low velocity location than in the high velocity location. That is the sense of things that you would expect if there is a sweeping of view volume with purge air.

KUYKENDAL: There appears to be another thing that leads us to that conclusion. The data rate that we would anticipate with the particle size concentrations that we think that we should have in that tunnel are much lower than the PILLS actually sees. It's only counting one in a thousand. The high concentration count should be about one in every 10.

BYERS: I think we ought to remember that we should not expect the same thing from the PILLS-IV and impactors--they're not measuring the same parameters.

KUYKENDAL: That's right. I want to make that point a little more emphatically. We didn't go into this test with the idea of establishing truth and then comparing the other instruments to that truth. We went into the tests with a great deal of background in the Andersen and Brink impactors. We wanted to see how the instruments that we had available would compare. If, for example, you could use the PILLS downstream of a control device and the Brink upstream and just compare the data? Well, I can safely say now that you can't. That is not to say that if you would take the PILLS and go upstream and downstream that that fractional efficiency would not have meaning. We are not prepared to make judgements at this time.

BYERS: Joe, could you quantify those high and low velocities?

MCCAIN: It was several meters and about a meter per second; I can't recall the exact numbers. I believe that the design value for the PILLS instrument was two meters per second or greater gas velocity. In the low velocity case it was definitely under the design value for the PILLS. I think that we were on the order of 8 in high velocity and about 1 in the low.

_____: What was the wind tunnel velocity?

KUYKENDAL: In our tests it was higher than that; it was 8.7.

SPARKS: We've just got time for one more question Bill.

SHE: Bill, how does your flow diameter compare with your total volume? Maybe the beam is seeing only part of the particle?

KUYKENDAL: The volume is cylindrical and I don't recall exact dimensions, but I expect I can check and let you know.

SPARKS: Thank you Bill.

ADVANCED PARTICLE SIZING TECHNIQUES

Pedro Lilienfeld, GCA Corporation

SPARKS: As Bill said, GCA has been working on some advanced methods of particle sizing and they have a representative here to talk about the work they've been doing.

LILIENFELD: We have a few areas in which we are working in the field and related to that which is of interest here. The description that Bill Kuykendal just gave with the tests bears upon one of these developments. The instrument that was shown previously is the result of a R&D program for EPA, in which we tried to develop an instrument capable of performing a real time size distribution measurement in emission sources, and the objective was to build an instrument that could be inserted into the stream and perform the measurement in the stack.

The general philosophy now is that this is probably the only reasonable way to perform any such measurements of size distribution and that extraction of samples for that purpose is probably unacceptable from all points of view. So the general approach we used for this device was to separate the particles collected at each stage by means of beta absorption as shown on one of the slides you saw before.

The original objective of the program was to be able to operate at temperatures typical of stacks--perhaps up to 500°F. The design of the device would be capable of withstanding that temperature. During the development of the instrument, it became apparent that there was one element that mitigated against being able to operate at elevated temperatures and that was the beta detector. The technology of beta detectors is somewhat of an alchemy at this point, especially as it refers to geiger tubes. Several manufacturers had assured us that there would be absolutely no problem in producing detectors that would operate up to 500°F.

We obtained such detectors and tested them up to those temperatures; they all failed within one hour. So, we pursued the development, but with the awareness that we would not be able to obtain a detector very easily that would stand the temperature of interest, and the device that was tested at EPA was essentially a room temperature device, perhaps 150-160°F. It served to demonstrate the principle, but the objective of operating in a stack had not been achieved. We are pursuing, at present, the idea of a so-called electronically quenched geiger tube beta detector, and that approach does show promise and perhaps that is going to be the solution.

As mentioned during Bill Kuykendal's talk, the substrate material which we used for these tests was mylar, but we have the intention of using a very fine stainless steel foil for high temperature tests. Of course, the mylar wouldn't take temperatures above about 200°F.

There were some interesting problems in the development of that device, and I'll just mention a few of them. One is the problem of coating the foil. This is an advancing foil, if that wasn't made clear before. It advances continuously and thus, the collection occurs in the form of a streak or line, and of course, we had to coat the substrate with some adhesive to be able to retain the particles. The problems of how to coat this foil and maintain the adhesive on while it is being unrolled and rolled over were things which we had to resolve and are more of a mechanical nature than anything else. We also had the intention of having the foil go back and forth, shifting sideways each time so that we could make a better utilization of the total area of the foil. We haven't gone this far in the present prototype, but we have the provisions to go the next step.

The signal from each of the seven impaction detection channels consists of two pulse trains from the two beta detectors at each stage, one the reference and the other one measuring the collected mass. These pulse trains are put into a frequency-to-voltage converter, each one of them and the outputs of these into a log-ratio module, the output of which is directly proportional to the mass concentration as measured by the device because of the moving nature of the substrate. Because of the different nozzle sizes required to impact different sizes at each stage, each of the streaks of collection was different in size and we had to adjust for that in terms of the detection geometry. We used a flow control that was based on running the last stage critical. We haven't evaluated entirely to what extent errors result from bounce off of particles at that last stage, of course, considering that by then the particles will only be the very smallest ones, a few tenths of a micrometer, and that bounce off under those conditions may be minimal although the velocity is high.

The system is built such that it can be inserted into a stack in a horizontal position and then lowered down by 90° once the device is inside. We felt that the use of any kind of bends or 90° nozzles or goosenecks was defeating the purpose of measuring the particulate size distribution representatively. I think the plot that Mr. Cass later on will present will bear out the problems associated with such 90° bends and gooseneck nozzles.

We are, at present, also in the course of developing another device for another group at EPA. That is what is possibly called the cheap version of that which was described previously. It is again the intention to measure the size distribution in real time and use inertial methods to separate the particles. The device that we are developing at present is a virtual impactor, a cascade virtual impactor, where we would use some means to detect or to evaluate the mass collected at each of these stages, perhaps, or possibly, a simple and very straightforward means of detecting. The three methods of detection of the collected, or the separated fractions at each stage are:

a) An optical method, by which we would measure the light transmission or extinction through each of the stagnation volumes of the virtual impactor.

b) Use of the electrical contact method on which the IKOR instrument is based, by extracting from each of the virtual impactor stages a so-called token flow and making that flow pass over a contact electric detector and deriving the signal from that and taking it to the outside.

I should clarify that the optical method that I mentioned before is based on the use of optical fiber bun[illegible] so that the sources and detectors would be on the outside of the chamber rather than on the inside exposed to the high temperature. Fiber-optic bundles can be made now in a very compact manner and this technique could be applied.

c) The third approach that appears at present as the most promising because of its simplicity and reliability is the measurement of the pressure drop developed across a filter collecting the mass of particulate at each stage with the token flow, as it is called, as the driving flow for that pressure drop.

That token flow is typically on the order of 1 to 10 percent of the sample flow and creates a small, low pressure drop across the filter at each of the virtual impactor stages. As a result of having this very low flow rate, these filters do not tend to clog and the collection occurs as a cake, such that the pressure drop versus mass per unit area is a very linear function over a very, very wide range. By differentiating the slope of this pressure drop increase across the filter, one can determine the concentration at any time. By simply measuring the pressure drop increase between two instants in time, one can determine what the average concentration was over that period.

This method appears extremely promising as I said because it is very simple and all that is required are some pressure taps carried to the outside of the stack and whatever pressure sensing or display devices on the outside, magnehelics or anything that one desires. And such an instrument can then be calibrated for the particular dust and the size characteristics of each stage. Our first indications are, that because of the size separation which occurs with the virtual impactor, the sensitivity to different types of particulates in terms of their pressure drop effects are lessened. That is, it is more a size function than a composition function. Of course, that will only apply to dry particles. When you get to liquid ones, that is going to change radically.

I have two viewgraphs, Figure 1 shows a schematic representation of a three-stage device as described above. The gas inlet is at the top and the virtual impactor consists of a conical stagnation volume. The first stage has its filter and we measure the pressure drop across it. The second stage is similar, and then the third stage, in this particular case, is a total filter. This device happens to have only three stages, but the technqiue can be extended, of course, to any number of them.

The next slide shows a typical run performed measuring just the pressure drop on two of these stages. We determined the mass collected on the filter at various intervals and it appeared as if the pressure drop increased linearly with the mass over this range. We're talking about concentrations on the order of somewhere between .05 and 0.1 grams per cubic meter. This particular run was performed with Arizona road dust.

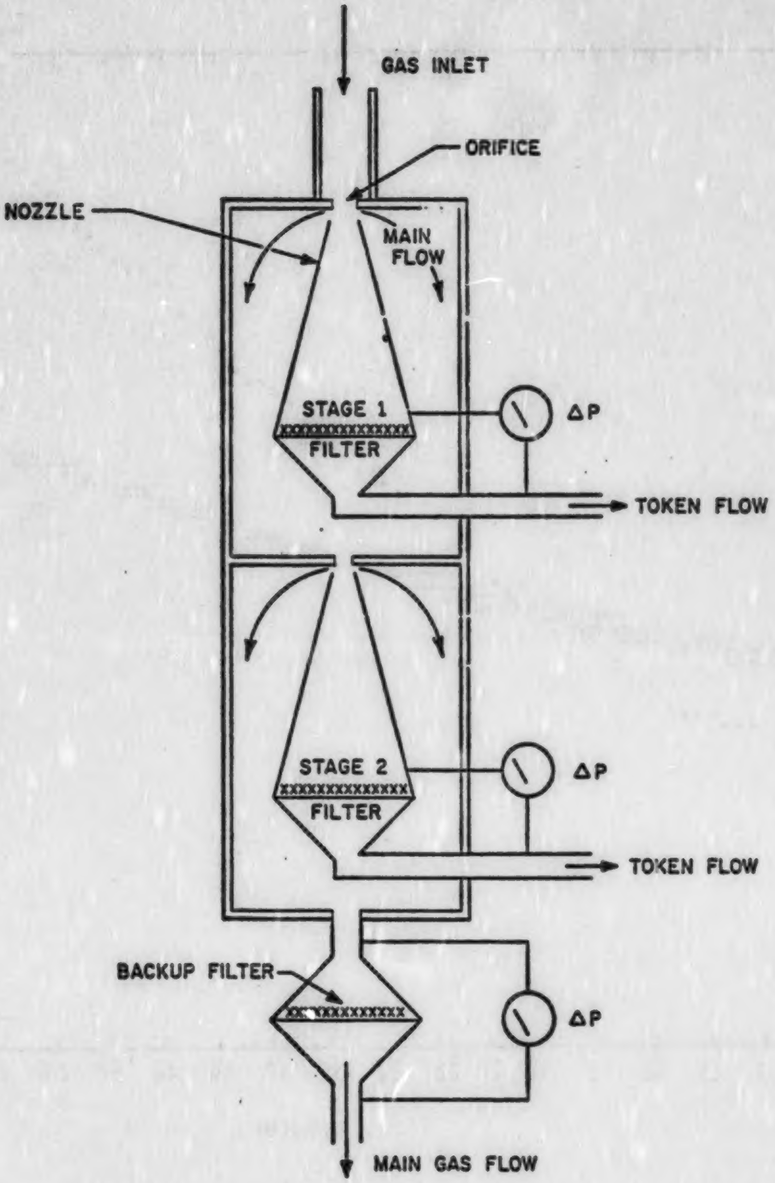

GAS INLET
ORIFICE
NOZZLE
MAIN FLOW
STAGE 1
FILTER
ΔP
TOKEN FLOW
STAGE 2
FILTER
ΔP
TOKEN FLOW
BACKUP FILTER
ΔP
MAIN GAS FLOW

Figure 1. Schematic representation of three-stage device.

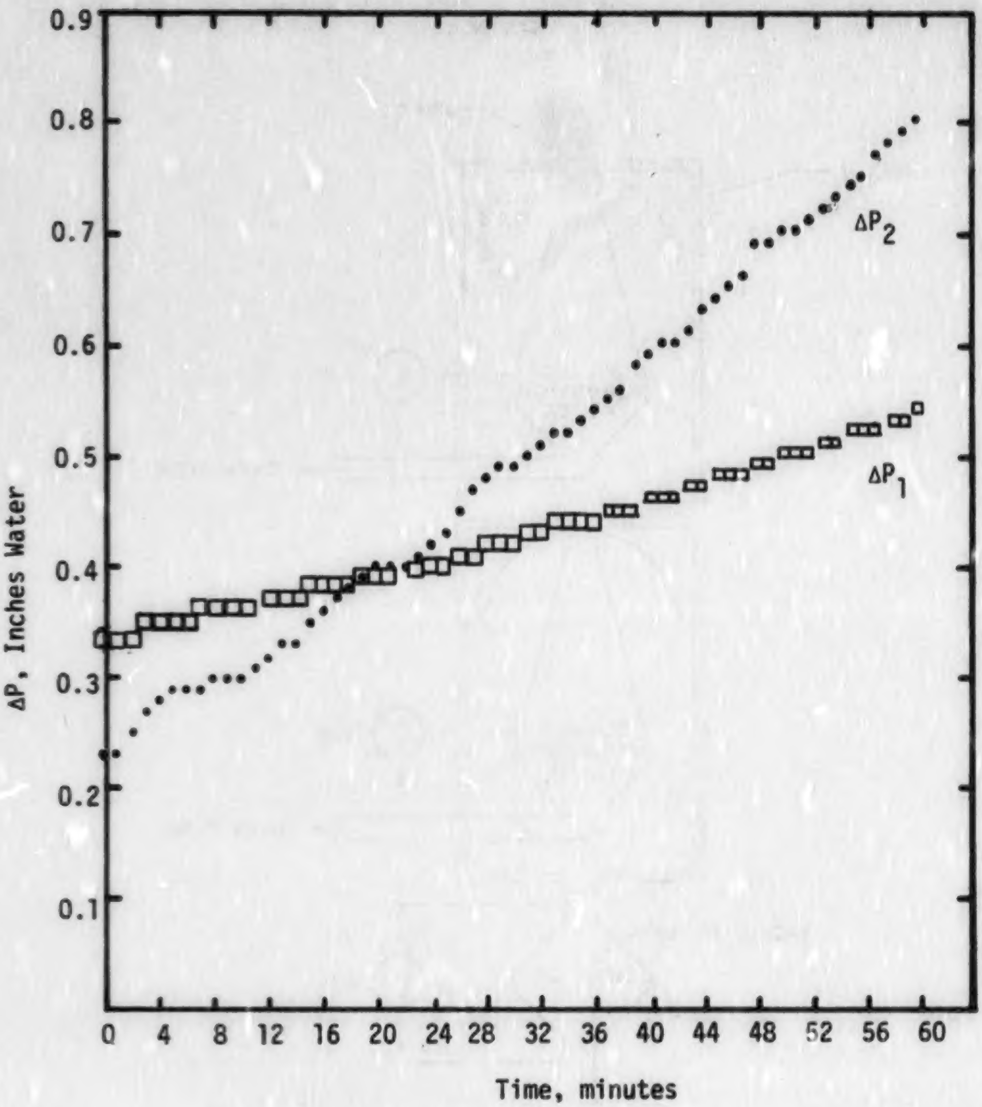


Figure 2. Data for test 23.

This particular development is of interest because it may afford a technique for sizing in real time with a minimum amount of effort and cost and yet relying on an inertial separation technique, which most probably is more acceptable than most other approacnes.

We have explored some of the problems that one faces in the design of virtual impactors, the main ones being losses at the nozzle exits and at the inlet of the stagnation volume. There are critical dimensions that one must maintain to minimize the losses and also the token flow itself has a very significant role in the degree to which the losses are important. We are in the process of optimizing this virtual impactor from all points of view, including the token flow and we are also in the process of writing a computer program that will take into account the effect of the token flow on the size distribution measurements. It is obvious that, if the token flow is more than about 1 percent of the main flow, the size distribution as measured by the device will no longer be rigorously that of the aerosol that one is trying to measure because a certain fraction is collected on each of the stages as a result of the token flow and not as a result of the inertial properties of the aerosol. But one can perform a mathematical inversion of the observed size distribution to come back to one with which the measurement has been made. We are in the process of developing a program, which, if it is rigorous must take into account the true shape of the cuts at each of the stages.

So far, we have found for the three stage device that I showed before that the losses can be brought down to the order of 5 to 6 percent by mass with respect to an independent probe. That is probably fairly acceptable for most applications. Another advantage of the virtual impactor as presented here is that, in addition to the pressure drop information, there is the mass information obtained on the filters themselves which can be weighed after the test has been completed. The amount of material that one can collect is very significant, it is not limited by bounce off and reentrainment as is typical in most jet-to-plate type impactors.

I want to mention one more area that we have been working which bears upon impaction, and that is work on the coating material for impaction substrates if a jet-to-plate device is used. This interest comes from our commercial instrumentation for the measurement of respirable dust in industrial environments. We found that the mechanism by which particles are collected on wetted or adhesive substrates is far more complex than was generally assumed. If one calculates the depth of penetration of a typical particle into a viscous substrate, one finds that, let's say for a 10 micrometer particle, the penetration is much less than the diameter of the particle itself. This applies to typical velocities, even up to sonic velocities and for typical viscosities of the coating. Consequently, one must conclude immediately that after a monolayer has been deposited, there should be no further action of the coating to retain the particles. But this is really not the case in practice. So we made a number of observations and measurements and determined that, depending on the type of coating one uses, there is a capillary type motion that tends to coat the particles immediately as they are being collected; that is, the upper surface of the collected layer is rewetted by capillary migration and thus provides an adhesive layer for further particles to be collected on.

The critical condition seems to be that if the rate of arrival, of collection by impaction of particles on the substrate exceeds the rate of capillary migration, then particle reentrainment and loss ensues immediately. However, if the rate of collection is less than the coating rate, then the particles tend to collect without significant losses. The problem here is obviously one of compromising between a coating that has low viscosity to permit this capillary migration to occur rapidly and the opposing requirement that it should have high viscosity to prevent cratering as a result of the air jet impacting on that coating, i.e., the coating should not be spread away from under the jet as a result of the impact of the air.

One has to deal with a number of complex phenomena and I don't want to go to deeply into these. The treatment of this subject involves the study of Bingham fluids, non-Newtonian flows, etc. But we have found that an interesting effect results from mixing a fairly fluid adhesive such as petroleum jelly and paraffin oil with a small amount of dry particulates, dry dust. This produces a rather unique mixture which exhibits a low viscosity in terms of capillary migration and a high viscosity on a macroscopic basis to prevent the cratering as a result of the jet impingement. This is work which is probably in some way relevant to all jet-to-plate impactors, and I think that further research should be done in that area.

I think that I have talked enough. Yes?

DISCUSSION

RAO: When you change the filter on the virtual impactors, how does the calibration affect you? Do you have to calibrate again?

LILIENFELD: You mean depending on what filter medium one uses? Or is that the question you asked?

RAO: Yes, or for the same filter media. If you change it, does the calibration change?

LILIENFELD: The calibration is essentially a function of the buildup of the particles. They build up as a cake, so the filter material itself, or the filter itself, plays a very secondary role. All it does is give you an initial pressure drop before the particles are collected, but that initial pressure drop is of no interest, it is the increase above that pressure drop that we're looking for. So the material of the filter has little relevance unless it is the type of filter that would prevent the formation of the cake. I don't know if that is the answer to your question.

OLIN: What concentration ratio were you able to achieve with the virtual impactor?

LILIENFELD: Concentration ratio?

OLIN: The ratio of the flow rate that goes through the receiver tube to the flow rate around.

LILIENFELD: The token flow fraction with respect to the total flow.

OLIN: Yes, do you call that the token flow?

LILIENFELD: No, the token flow is that secondary flow one extracts through the stagnation volume. We have explored fractions between 1 and about 12 percent. We have found that the losses seem to decrease, the nozzle losses seem to decrease as you increase the token flow.

OLIN: That's been the observation of LBL and ERC. One to seven is what they claim.

LILIENFELD: Yes, I know they have. Well, there is apparently not an optimal ratio. Because so far it is a monotonic behavior. That is, we increase the token flow and we see decreasing losses. However, at some point, you have to compromise. You have to stop because the errors associated with collecting particulates resulting from the token flow then become too significant.

OLIN: I have one more question. How often would you have to change the filter in the token flow?

LILIENFELD: Well, that depends, of course, very much on the concentration and it depends on the dimensions of the filter and it depends on the token flow that you end up having. We calculated that, for concentrations on the order of 10 grams per cubic meter, for typical situations you could run the filter for perhaps one or two hours without having to change it. If you make the filter larger than a few centimeters in diameter, perhaps you can run 8 to 10 hours.

OLIN: In an automatic system you would have to have some kind of a tape feed.

LILIENFELD: Well, that depends on how often you do want to change it. If you're concerned about running let's say for a whole day, without changing anything, you can do that without advancing anything. It may be easier to change the filters once a day then to have an advance mechanism which is complex and can break down.

_____: If you change filters, using the same type of filter, but put a new filter on, is the calibration mass sensitivity calibration the same?

LILIENFELD: Yes, that is true.

_____: So any variations in the filter have no effect.

LILIENFELD: No, it is a totally negligible effect. As a matter of fact, we have tried different types of filter and have found very little effect on the average. As long as the filter does not have significant depth, to the degree that particles would be collected by two different mechanisms, first by penetration into the filter, and then by the formation of a cake. Then you would see a discontinuity in the curve of the pressure drop versus collection. But if you use any type of tight filter, Teflon filters for example would be applicable in this case for high temperatures, you don't have to worry.

_____: So you use membrane type filters?

LILIENFELD: We also use the glass fiber filters, and in that case, you may have a very short initial difference in the behavior when the particles are penetrating into the fibers. As soon as the cake forms, then you get back to the original calibration, which then is the same as the membrane filter.

ENSOR: How well do you need to know your pressure drop to obtain a reasonable resolution in your concentration?

LILIENFELD: That depends on a number of factors. It depends on the concentration. If the concentration is high, the rate of change of pressure drop is high too.

ENSOR: Well, I understand that. What range are you talking about?

LILIENFELD: You're talking typically in the range of an inch of water per hour, or so.

SMITH: What does the stage collection efficiency curve look like for a virtual impactor?

LILIENFELD: It is very similar to a jet-to-plate impactor. It is slightly less sharp and has a rather different impaction parameter. It's shifted for the same dimensions, it is shifted with respect to that which is obtained for the jet-to-plate impactor.

_____: Is it necessary to introduce your token flow as a part of the main gas stream or can you introduce it from the outside so you don't tend to extract part of your sample stream?

LILIENFELD: It appears that it is necessary to extract it as a fraction of the total flow, because it facilitates and provides a general drift of the particles into the stagnation volume.

_____: I was thinking in terms of an annulus that would have the air introduced there to give you the same flow.

LILIENFELD: Well, it may be possible to come up with a geometry with comparable effects. I don't really know.

SPARKS: Thanks, Pedro.

LIGHT-SCATTERING PARTICLE SIZING TECHNIQUES

Dr. C. Y. She, Dept. of Physics, Colorado State University

SPARKS: Colorado State University has been working on a grant on optical techniques for particle sizing, and Dr. She of Colorado State University is here to talk about how that works.

SHE: I would like to say, first of all, that I have enjoyed this very successful conference very much. Very informative indeed! I thought maybe in addition to a brief progress report on the six-month old program at Colorado State, I would comment on the light-scattering technique in general and share with you my opinion of the potential of this technique.

When I asked myself what one would like in particle sizing, and being an inexperienced worker in this field, I decided (Figure 1) that perhaps one would like to do it in real time and be able to measure the particle size without the knowledge of other properties, such as the refractive index in the optical case. If possible, one would like to be able to process a lot of particles, in other words, to handle a high concentration of particles. Fourthly, one would like to be able to size the particles in a wide range, mainly from submicron to 10 micron, if that is possible.

One can think of light-scattering techniques simply as follows. One should have a light source, maybe a laser in this case, and focus it down to a volume V. Now, send the particles through this focal volume and you will see a current pulse on the detector. Hopefully, by measuring the height of the pulse you learn something about the particle size.

In fact, all of the commercially available devices, besides the PILLS that Dr. Kuykendal talked about today, use a single detector arrangement like that. Of course, the height of the current pulse you detect depends not only on the particle size, but also on the refractive index. Therefore, there may be some ambiguity with this kind of arrangement. One way to get around this problem is to use two detectors as Dr. Kuykendal has shown, and there you take the current ratio and it turns out that the current ratio in the forward direction is more or less independent of refractive

I. IN REAL-TIME

II. INDEPENDENT OF REFRACTIVE INDEX

III. WITH HIGH CONCENTRATION

IV. OVER A WIDE RANGE OF SIZES

- Light scattering satisfies (I) better than other techniques
- The intensity-ratio technique, in particular, satisfies both (I) and (II); it has potential to be better in (III) and (IV).

Figure 1. Requirements for sizing.

index. One thing you want to make sure of is that you always have single particle detection. You want to avoid multiple particles. You can see, if you have a concentration n, the number of particles in the focal volume on the average would be $\overline{N}$, which equals n times the viewing volume V. If you want to avoid multiple particle scattering, you would limit $\overline{N}$ to much less than one, thereby restricting the detectable concentration to be less than the inverse of the focal volume you can achieve.

If you want to be able to exceed this limit of detectable concentration, you have to come up with a technique that will allow you to detect multiple particles. We have some ideas along these lines, and I'll be talking about them.

The range of sizable particles is limited by the theory of scattering. In general, there is not much one can do, but I will also breifly talk about some methods by which perhaps one can expand that range a little more.

The second viewgraph (Figure 2) is a schematic that looks quite similar to the one Bill [Kuykendal] used. You have two detectors here at different angles; you amplify the current pulses, take the ratio by dividing, and then send that divided pulse into a pulse height analyzer or something like it. As a result, you will get a particle size distribution as shown. On the X-axis you have current ratio, which indicates the size of the particle, and on the vertical axis you have the number of counts corresponding to a particular pulse height. A curve like that, with proper calibration, will give you directly the particle size distribution in real time.

The two-angle intensity ratio technique was first pointed out by Hodkinson in a paper in Applied Optics around the early 60's. He pointed out that if you take the ratio, the result will be independent of refractive index. The intensity ratio is plotted here versus particle size (Figure 3). This parameter alpha, is the ratio of the circumference, π times the diameter of the particle, divided by the wavelength you use. The range of particle sizes that you can measure depends on the two angles chosen. If you choose, for example, the 10° and 5° angles in the forward direction, you are dealing with the curve marked 10°:5° on Figure 3. The other abcissa scale is

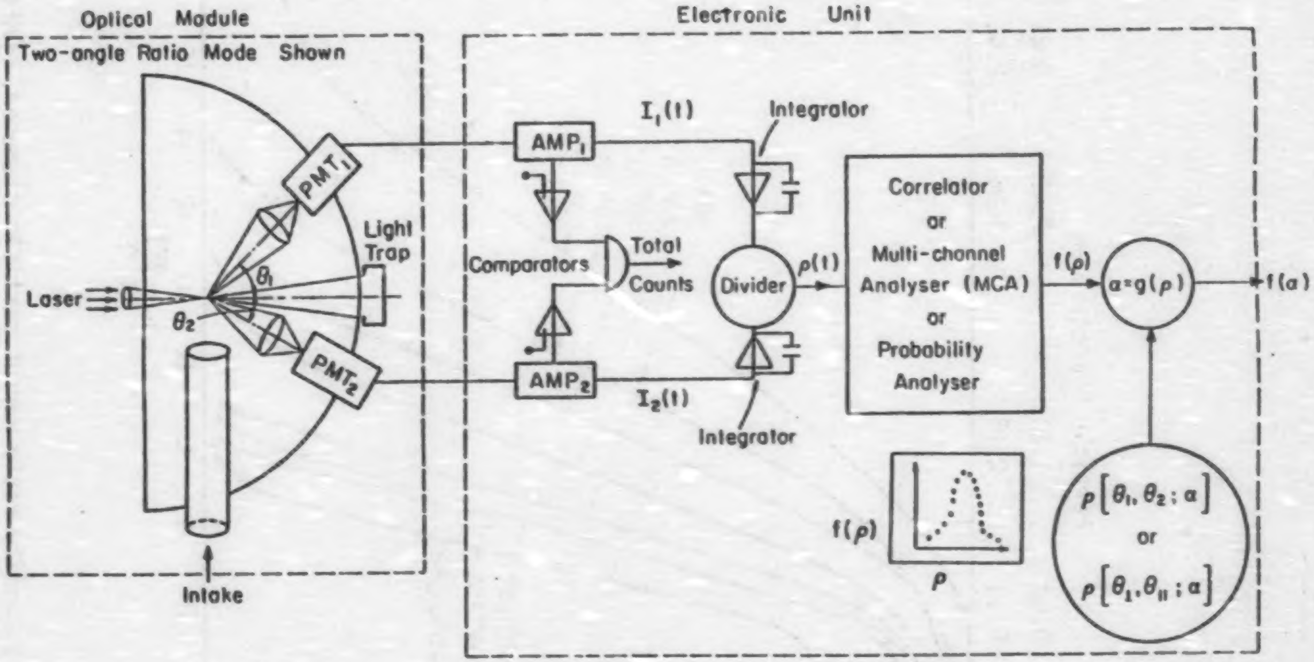

Optical Module
Two-angle Ratio Mode Shown
Laser
θ_1
θ_2
PMT$_1$
PMT$_2$
Light Trap
Intake
Electronic Unit
AMP$_1$
AMP$_2$
$I_1(t)$
$I_2(t)$
Comparators
Total Counts
Integrator
Integrator
Divider
$\rho(t)$
Correlator or Multi-channel Analyser (MCA) or Probability Analyser
$f(\rho)$
$a=g(\rho)$
$f(a)$
$f(\rho)$
ρ
$\rho[\theta_1, \theta_2 ; a]$ or $\rho[\theta_\perp, \theta_\parallel ; a]$

Figure 2. Schematic of an optical particle counter.

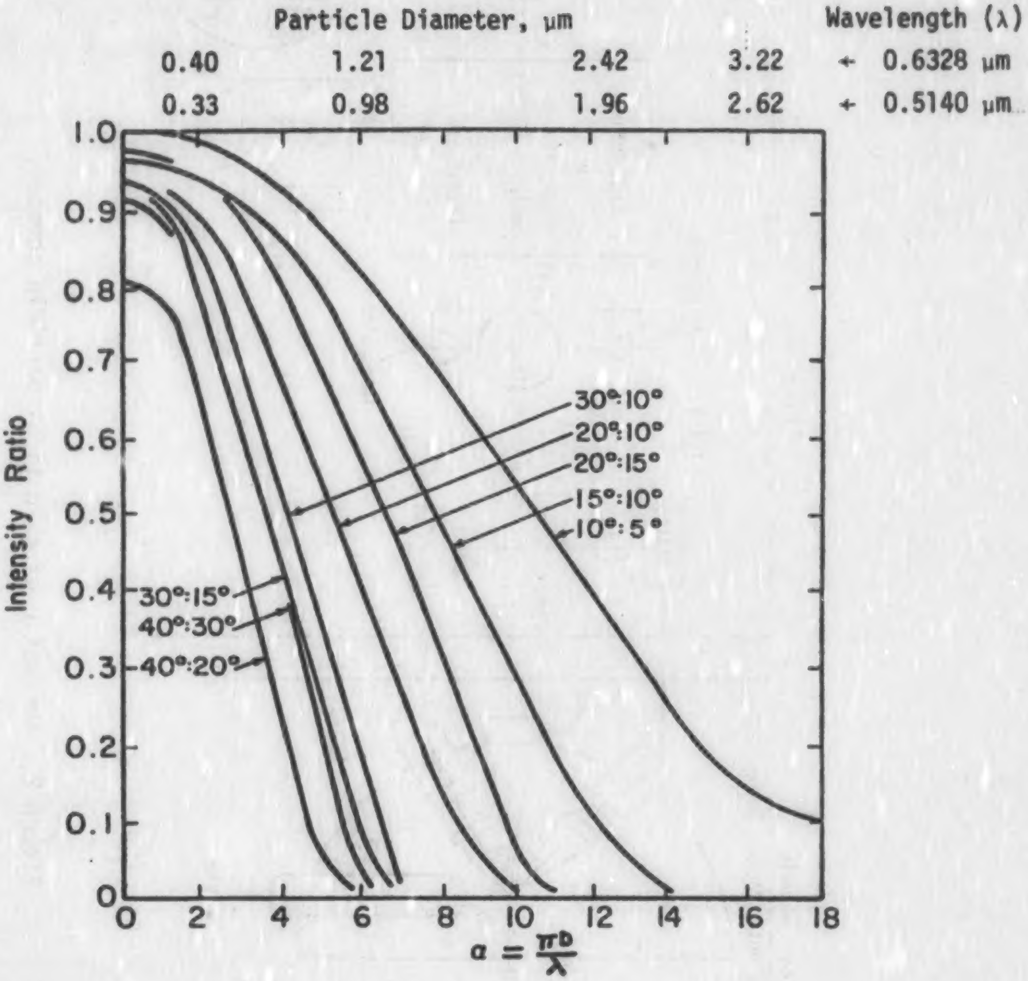


Figure 3. Intensity ratio versus particle diameter. [After Hodkinson].

particle diameter at particular wavelengths. This corresponds to 0.4 microns, for example, if you use a Helium-Neon laser at λ=0.6328 µm, to say something like 3 microns. If you want to extend the range to larger particles, you have to be able to detect scattering at smaller angles. If you want to size smaller particles, you want to have a technique to senstively measure the small changes in the intensity ratios and that, of course, is not very easy.

This viewgraph (Figure 4) is taken from a paper by Kreikebaum and Shofner. They designed the PILLS IV. They plot several curves similar to the one that I showed; except in this case, they take into account the complex part of the refractive index. Of course, for most particles of interest, you are dealing with the complex refractive indices, and they have shown, more or less, that the ratio technique is also insensitive to that. In fact, if your particles are somewhat glossy and have complex indices, you can get a smoother curve than those corresponding to particles with real refractive index. Looking at this sort of curve, you would conclude that the accuracy of particle sizing by this technique is around 10 percent independent of the knowledge of refractive index.

ENSOR: What's the vertical axis on that graph?

SHE: The scattered intensity ratio.

This viewgraph (Figure 5) summarizes the achievement up to this point. As I said, Hodkinson was the one who pointed out this technique theoretically, and in fact, a fellow by the name of Gravatt at NBS has done considerable work to put this idea into practice over the last few years. He was able to size, by using different lasers, particles from 0.2 to 4 microns. He was using 10° and 5° as compared to the PILLS IV which uses 14° and 7°. The focal volume Gravatt selected was 1.5×10^{-5} cc. An improvement, you see, of the PILLS IV over Gravatt's is basically this. They think that they can work with a smaller focal volume like 2×10^{-7} cc, to size higher concentrations. The concentration that the PILLS IV claims to be able to handle is 10^6 per cc, but Gravatt, working on a research program at that time, did not try to make a very small focal volume. He could count only 10^4 particles per cc. I want to point out that if you multiply the

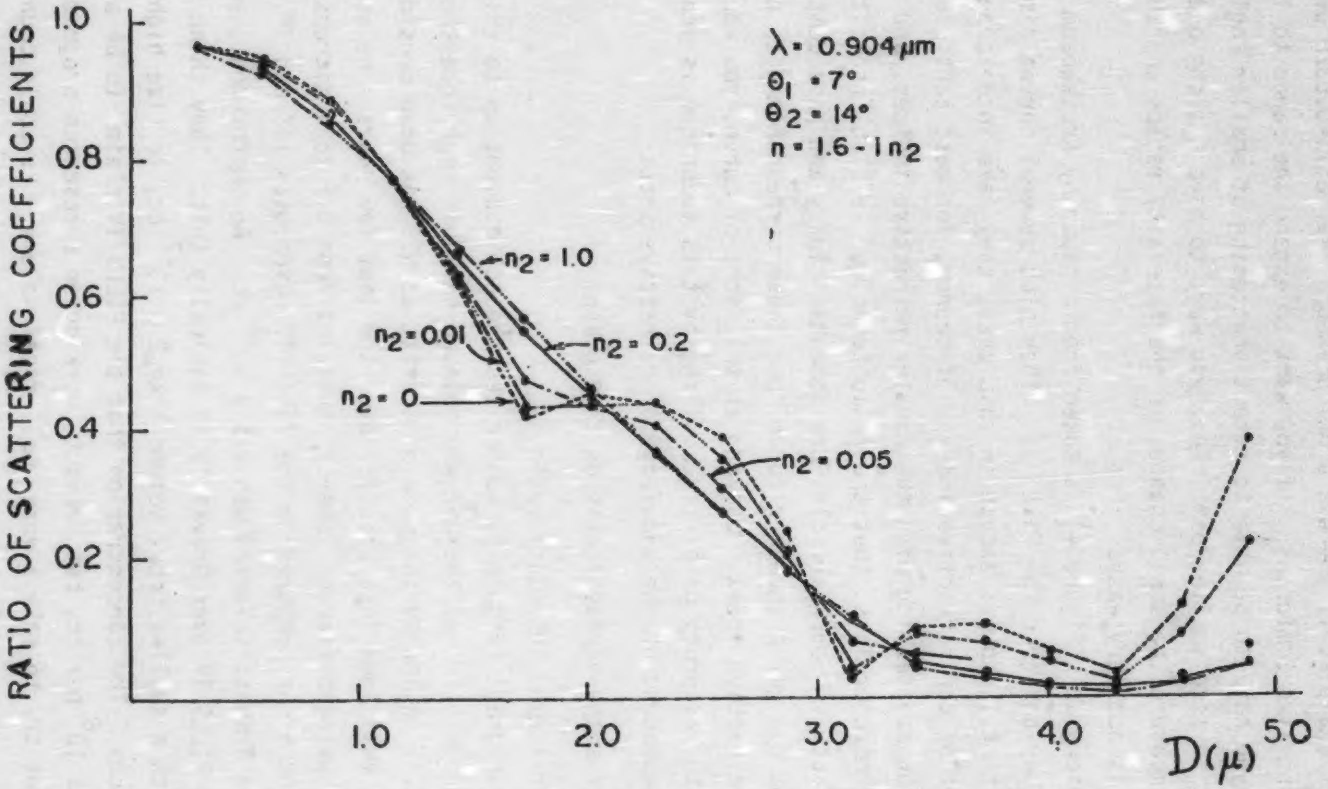


Figure 4. Scattered light intensity ratio for particles of varying diameter and refractive index. [After Kreikebaum and Shofner].

	GRAVATT	KREIKEBAUM and SHOFNER
Range	0.2-4.0 μm	0.2-3.0 μm
Angles	10°/5° etc.	14°/5°
Beam Dia.	0.14 mm	0.02 mm
Depth of View	1.0 mm	~ 0.6 mm
Viewing Vol.	1.5×10^{-5} cc	2×10^{-7}cc
n	10^{4}/cc	10^{6}/cc
$\overline{N}$	0.15	0.20
T_t	10 μs	~ 2 μs

Improvements Desired

1. Increase the detectable concentration n.
2. Increase the sizeable range.

Figure 5. Comparison of Gravatt and PILLS-IV optical particle counters.

concentration with the focal volume, in both these cases, you get a number like $\overline{N} = 0.1$ or 0.2. So, on the average, you have much less than one particle in the viewing volume. This is done purposely to avoid the problems of multiple scattering. If you want to do better, than you have to be able to handle two overlapping pulses. That is the only way you can increase the detectable concentration. Therefore, improvements are possible: you want to be able to increase $\overline{N}$, and you want to extend the sizable range also.

Let's look at the lower part of the viewgraph (Figure 6) first. Let's assume that you somehow get two particles into the viewing volume at the same time. Two pulses like that; they overlap one another. If you devise electronics in such a way to sample the height of the pulses only when the slope of the pulses change signs, in other words when the slope changes from positive to negative, you measure the height of pulses for a short duration of time. If you indeed have such electronics then you can separate two pulses as shown. Therefore, two overlapping pulses cause a problem no longer, and you only have to worry about three overlapping pulses. So, instead of $\overline{N} = 0.2$, such as in PILLS IV, you can now handle $\overline{N} = 0.6$. This is a problem of statistics. The probability of having three particles for $\overline{N} = 0.6$ is about 2 percent which you can afford to lose. Similarly, in the PILLS' case, they try to avoid two overlapping particles. When $\overline{N} = 0.2$, the probability of having two overlapping particles is also about 2 percent. Basically if you can handle two overlapping pulses, you can push the value of the $\overline{N}$ from .2 to .6 and that is a factor of three improvement. I will show you an electronic circuit that can do that.

Looking ahead, there is really no fundamental reason that one cannot push $\overline{N}$ even higher. Suppose you know how to separate six overlapping pulses. The only thing that you have to worry about is if you have seven overlapping pulses, or higher. In that case, you can handle $\overline{N}$ of three and that would increase the detectable concentration by a factor of 15. I think that we will be quite happy to achieve that now; if you can do better, you can increase the concentration even more.

Here is an electronics circuit (Figure 7) that we use to handle two pulses. Basically, we have two photomultipliers, integrators, pulse inverters, and a divider. We send the divided pulse into a pulse height

Number of Particles in Focal Volume	Probability of n particles entering viewing volume simultaneously, P(n)			
($\bar{N}$)	P(0)	P(1)	P(2)	P(3)
1	3.68×10^{-1}	3.68×10^{-1}	1.84×10^{-1}	6.13×10^{-2}
0.8	4.49×10^{-1}	3.59×10^{-1}	1.44×10^{-1}	3.83×10^{-2}
0.6	5.49×10^{-1}	3.29×10^{-1}	9.88×10^{-2}	1.98×10^{-2}
0.3	7.41×10^{-1}	2.22×10^{-1}	3.33×10^{-2}	3.33×10^{-3}
0.2	8.19×10^{-1}	1.64×10^{-1}	1.64×10^{-2}	1.09×10^{-3}
0.1	9.05×10^{-1}	9.05×10^{-2}	4.52×10^{-3}	1.51×10^{-4}

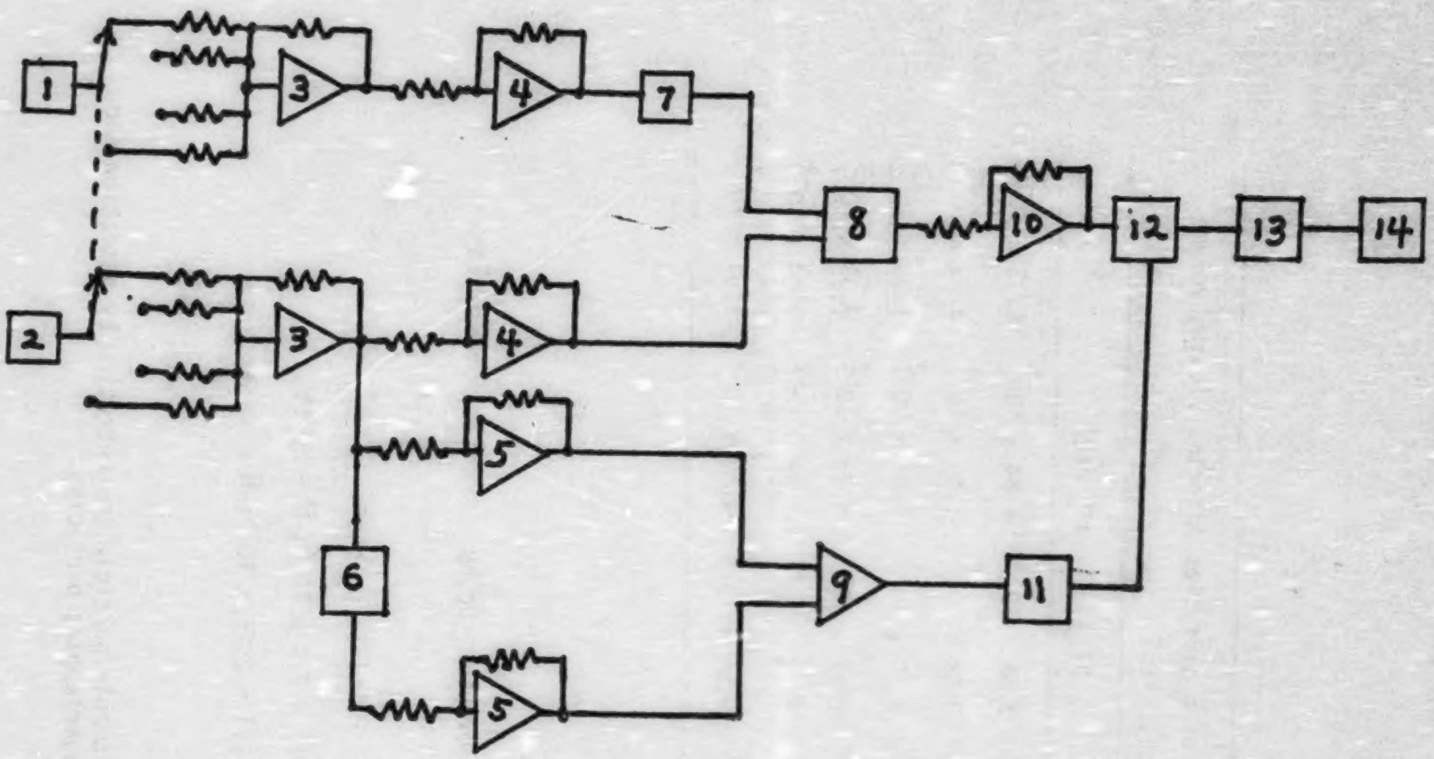
Detection of two overlapping pulses will increase $\bar{N}$ by a factor of 3.

Why not detecting multiple pulses?

For $\bar{N} = 3$, $P(7) = 2.6 \times 10^{-2}$; $\bar{N}$ ↑ by 15

For $\bar{N} = 6$, $P(11) = 2.25 \times 10^{-2}$; $\bar{N}$ ↑ by 30

Figure 6. Increase of allowable particle concentration by detection and separation of overlapping particles.



1. Photomultiplier at 5°
2. Photomultiplier at 10°
3. Input amplifier
4. Inverting amplifier
5. Isolation amplifier
6. Signal delay circuit
7. Denominator bias circuit
8. Analog divider
9. Comparator
10. Inverter and attenuator
11. One shot
12. Electronic sampling switch
13. Impedance matching amplifier
14. Pulse heighth analyzer

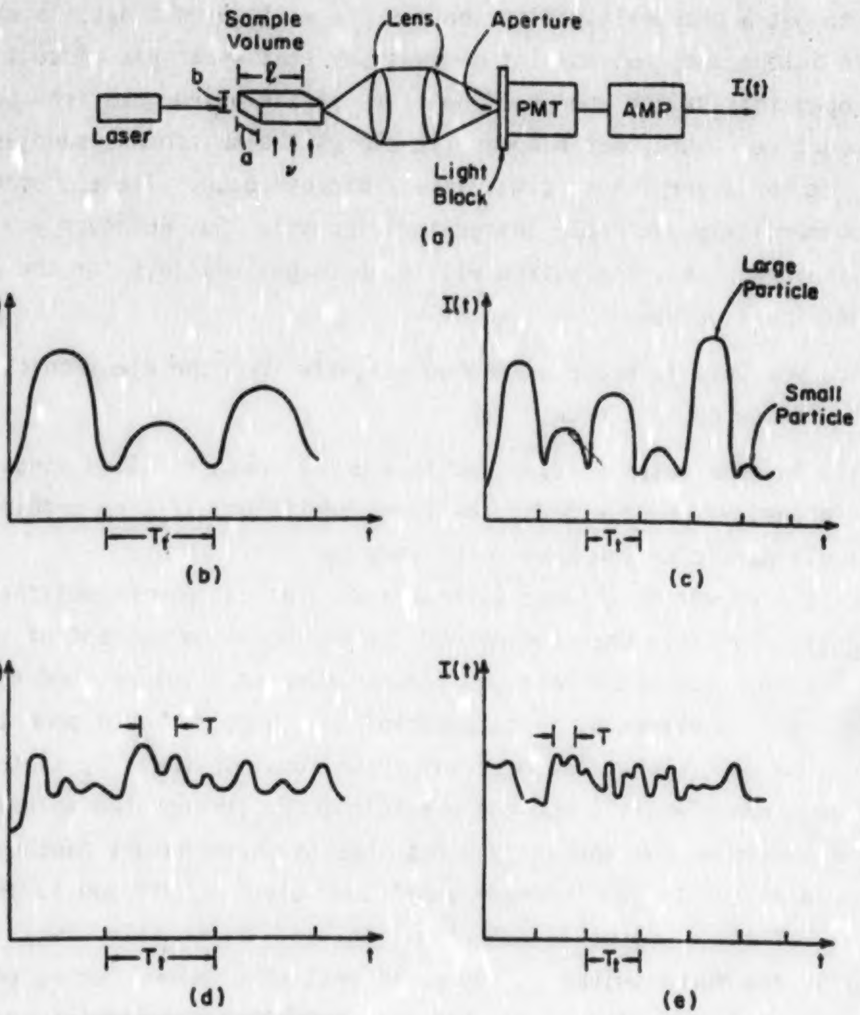
Figure 7. Circuit diagram of two pulse equipment.

analyzer to get a probability distribution. I will go into details when people are interested, but now let me just say that what this circuit will do is to open this switch whenever the slope of the pulse goes from positive to negative. When that happens, it closes the switch and samples the pulse height for a very short time, like 2 microseconds. These elements are all commercially available integrated circuits. So, whenever you have two overlapping pulses, the switch will open to get readings for the height of the individual pulses.

_____: Are you able to get clean enough signals from the electronics to handle it? Noise doesn't bother it?

SHE: Well, in some cases it does and in some it doesn't. It depends on how much intensity you have. For the large particles, it's no problem; for the small particles you have to be very careful.

This is a viewgraph (Figure 8) from a theoretical paper published in Applied Optics by Steve Chan and myself. This shows the concept of multiple particle sizing. You see a laser is focused down to a volume, and the scattered light is viewed by a photomultiplier. Suppose for a moment you only allow one particle at the most into your focal volume. T_t is the transit time, the time it takes the particle to go through the volume. Whenever a particle goes through you get a pulse whose height depends on the particle size. If you increase your flow velocity, all you do is to squeeze all of these pulses together in time, but if you allow, say, three particles in the focal volume at any given instant of time, during transit time T_t, you will see not just one big peak but three overlapping peaks of various magnitudes. So, the wiggles in the lower figures correspond to particles. The only requirement is that the time between two successive ripples should be longer than the resolution time of the electronics T_r. In other words, if the change here is slower than what your electronics can resolve, then you can handle a higher particle concentration. In order to process this, you need to be able to handle multiple pulses. What you gain is a ratio of T_t/T_R, which may be a factor of 10 or so. This is very nice conceptually, but if you use this technique without care, for real particle sizing or sizing in real time, you are in trouble because the height you measure then is not necessarily the same as the original height of the pulse.


Sample Volume
Lens
Aperture
b
ℓ
I(t)
Laser
PMT
AMP
a
v
Light Block
(a)
I(t)
T_f
t
(b)
I(t)
Large Particle
Small Particle
T_f
t
(c)
I(t)
T
T_f
t
(d)
I(t)
T
T_f
t
(e)

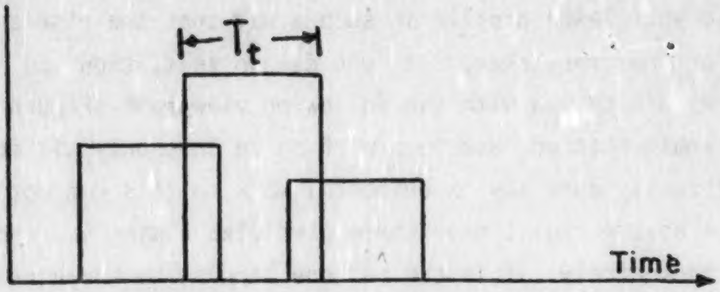
- a. Experimental arrangement.
- b. Current pulses for a modest flow speed.
- c. Current pulses for an increased flow speed.
- d. Viewing several flowing particles in the scattering volume.
- e. Viewing several flowing particles in the scattering volume at increased speed.

Figure 8. Multiple particle sizing.

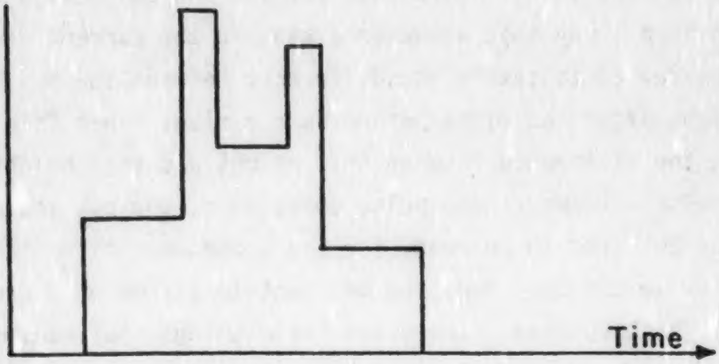
So you have to do some fancy footwork, so to speak. What you could do is to shape your laser profile in such a way that the rise and drop of the pulse becomes very sharp. If you can do that, then you are all right. Let me show this to you with the following viewgraph (Figure 9a). Let me emphasize again that we have been working on this only six months so that we haven't really done any experimental work on this concept.

Let me assume that I have three particles coming in, and I somehow can shape them nicely. This can be done perhaps by aperaturing the laser profile or sending the laser beam through some optical fibers which, I don't know for sure at this moment. The overlapping pulses will now give you something like this (Figure 9b). If you can now design an electronic circuit in such a way that whenever a rise in the current is confronted, you ask the device to sample the difference between the heights of the current right after and right before such a rise. When this goes up again, you sample the difference between this height and that height and you get a second pulse. Whenever the pulse comes down, you ask the device not to do anything but just to be ready for the subsequent rise of the current. This we know we can do. Now, you may want to ask me what are the limitations of this sharp rise. There are three things that can limit this: one is how sharp you can make your laser beam profile, the second is the resolution time of the electronics, and the third is the size of the particle. When a particle enters the focal volume, depending on the diameter of the particle, it takes a finite time to enter the viewing region and that will also smooth out the sharp rise. The longest of these times will determine the rise time in question. So, the number of multiple particles you can sample equals the ratio T_t/T_R. I think 5 or 10 is about right for this ratio.

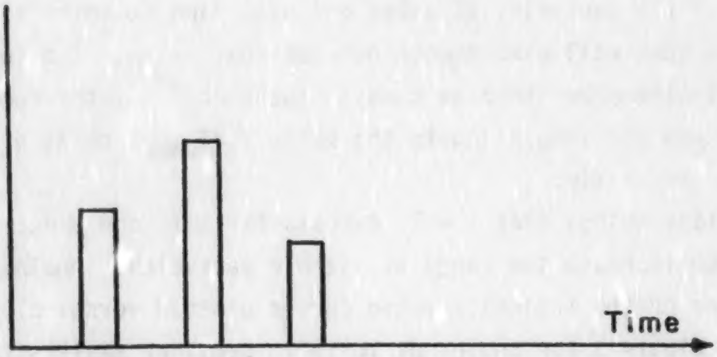
The last things that I will discuss for this conference are some possible ways to increase the range of sizable particles. Again, (Figure 10) this is one of the intensity ratio curves plotted versus alpha, which I showed in Figure 3 for angles of 10°:5°. Consider small size particles, perhaps an alpha of 2, which corresponds to 0.4 microns for helium-neon lasers and 0.3 for Argon-ion lasers. In order to sense this portion of the curve,



(a) Individual Overlapping Pulses



(b) Sum of Overlapping Pulses

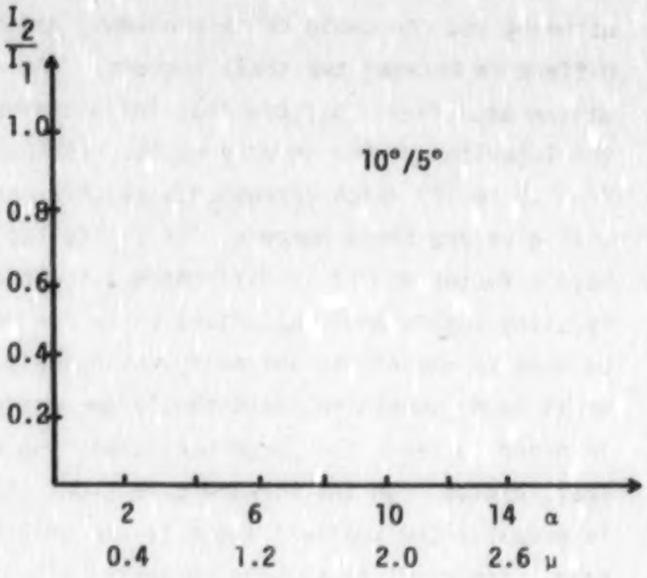


(c) Retrieved Pulse Height

Figure 9. Determination of true pulse height.

$\frac{I_2}{I_1}$	$\frac{I_1}{I_2}$	$\ln \frac{I_1}{I_2}$
0.99	1.01	1.01×10^{-2}
0.95	1.05	5.13×10^{-2}
0.90	1.11	10.5×10^{-2}

Will help small α.



λ = .6328 μ

BACKSCATTERING

5°/10° ratio is possible in the backward direction.

This will help large particles.

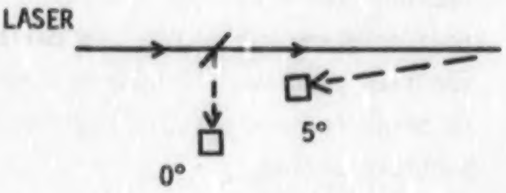


Figure 10. Possible ways to increase the sizeable particle range.

assuming you can avoid noise problems, you have to be able to read the difference between two small numbers. You can help that by using a logarithm amplifier. Suppors that instead of measuring this ratio you measure the logarithm of the inverse ratio. Taking, for example, the ratios from .9 to .99 which corresponds to this part of the curve, their inverse will give you these numbers. If I take the logarithm after that, you have a factor now of 10 difference with which to work. In other words, by using logarithmic amplifiers which are commercially available, you might be able to expand the intensity-ratio scale. Another thing that one can do is to do something about the large particle range here. Remember that, in order to sense the large particles, you want to be able to work with small angles. In the forward direction, this is very difficult; 5° is probably the smallest angle to use unless you want to detect the laser beam. You might be able to do back scattering, however. You can shine your laser beam through a beam splitter and focus it down onto particles. You put your two photodetectors, for example, at 5° and at 0°. The back scattered light will come and hit this beam splitter and go this way. That will not mix with the forward laser beam. That way you can do a ratio of 5°:0°. Then, you can, perhaps, hope to do better with large particles.

That is all of the viewgraphs that I have. Let me summarize by saying that we have been able, in the laboratory, to separate the two overlapping pluses and get a particle size distribution, even though we have some problems with noise and calibration. We don't have a particle generator like you have. I hope my presentation will give you experts a chance to think seriously about light scattering as a viable technique for particle sizing.

DISCUSSION

ENSOR: Just a comment. Generally, light-scattering is more sensitvie to particle shape than to refractive index; this may reduce its practicality.

SHE: I don't know about that. There was a paper sent to me, after we published that *Applied Optics* paper, by Brinkworth of England. They show that backward scattering, really backward, is as good as forward in terms of its insensitivity to the refractive index. If you think about it, perhaps it is not too surprising.

ENSOR: Well, the imaginary refractive index should be more...

SHE: Perhaps. I don't know.

ENSOR: And lidar is used in cloud physics to tell whether you have ice crystals or snow or like that.

SHE: That's something that one could consider. Of course, back scattering has less intensity, but if you are working with large particles, you are all right. For small particles, the backward and forward intensities are comparable.

BOLL: If I remember rightly, Hodkinson did his work based on the assumption of uniform spherical particles. Flyash particles tend to have a certain amount of shell structure to them, because as the gas cools down, you get things condensing out on the surface and while it would seem to be a small effect, I was wondering if you had given any thought to how it would affect those calibration curves.

SHE: I didn't do any work myself. Gravatt did look into some such particles. Let me say, first of all, in light-scattering we always assume spherical particles. Other shapes are quite complicated even though, in principle, you can determine different particle shapes. In practice, it is not easy. However, Gravatt did check the particle shape question in sizing and found that it is not that sensitive to shapes if you can tolerate the accuracy of about 10 percent.

BOLL: I'm speaking of refractive index variations in rate.

SHE: That's a good question. That may or may not be important. I haven't done any work. If the particle is much smaller than the wavelength, certainly it doesn't make much difference.

BOLL: I was thinking that the PILLS data were more compact and wondering why. Maybe that could be the explanation.

SHE: Could be. That's the first time I saw the data also.

ENSOR: I would like to comment here that Meteorology Research has been using different approaches in applied light-scattering. I feel that applied light scattering is one of the forgotten aspects of engineering. However, we, in general, have been using across-the-stack techniques and inverting the information into forward light-scattering angles, and with proper signal processing you get more information about the size distribution. Our specific objective is to try to get a better measurement of aerosol mass and aerosol volume. I might point out that there are other techniques available.

SHE: Yes, well I guess that everybody knows that light-scattering techniques measure different diameters than those measured by impactors.

ENSOR: Well, there is a lot of information on light-scattering around.

SHE: It is difficult to get it all out.

SPARKS: Thank you.

GENERAL DISCUSSION OF CASCADE IMPACTORS OR ALTERNATIVE DEVICES

D. B. Harris, Process Measurements Branch, IERL-RTP

HARRIS: The only alternatives that I want to discuss right now are our attempts to use the Series Cyclones system to give us particle size distributions. You have seen some of the results as far as their field useability goes in the work that Bill Kuykendal presented earlier this morning. I would just like to go into it a little bit to show the calibration end of things and what we're hoping to apply these devices to. The main reason we looked at using Series Cyclones instead of impactors is that we were having to supply sufficient material to other people to do testing for chemical or biological health effects purposes. When you get to the point where you have a 10 milligram collection per stage and one of these guys comes and says to you, "I need a gram to work with," you don't look forward to running impactors for the rest of your life to get enough sample for him.

The Series Cyclones look like our only alternative. The device that I brought in yesterday (Figure 1) was our first attempt at it. The system operated at 1 cfm* and had three sizing cuts to give us particles in the range of about 3 microns maximum size. Figure 2 is a set of curves of collection efficiencies of these devices. Their shape is why I made the comment that it looks like an impactor collection efficiency curve. The classical cyclone curves usually have less slope in the straight portion of the curve. The cut point is not as sharp. As a matter of fact, some of them look better than you get on some impactor data. Data from five cyclones are shown in Figure 2. Two of them are units that were designed four years ago for us by McCrone Associates. They were arranged in a parallel configuration as we were attempting to develop a sizing scheme that would allow us to do isokinetic sampling while running parallel cyclones and the bypass filter, adjusting the filter to meet the isokinetic requirements. Subsequently, Southern used two of these designs in developing the Series Cyclones, adding

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*See metric conversions listed on page 252.

Figure 1. One ACFM series cyclone showing the three cyclones and Gelman back-up filter in the preferred orientation. The nozzles which were developed for isokinetic sampling from 10 fps to 100 fps are also shown.

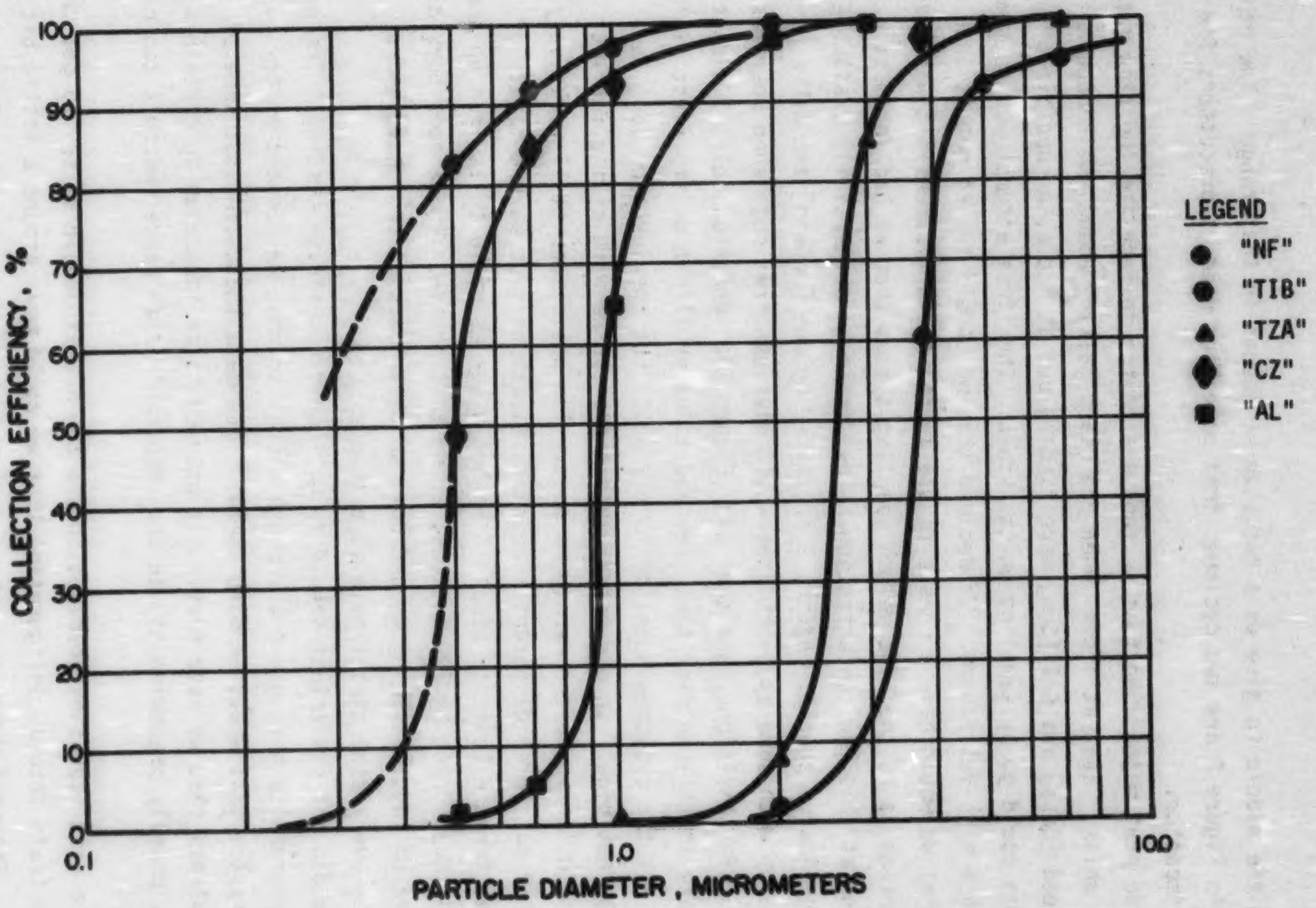


Figure 2. Collection efficiency versus particle diameter. (5 cyclones, 1.0 ACFM, ρ = 1.35 g/cc, 22 °C, 29.5 "Hg).

one in the middle to give us a cut size we needed in that range. The others shown in Figure 2 are two cyclones that have been used as precutters for the Brink impactor.

The next major step that we took was to try to increase the flow rate of the units. Evan at 1 cfm, when you start talking about the concentrations that Reed [Cass] and Gil [Sem] were finding out at Nucla, a good bit of your lifetime could go by sampling on the stack. The first attempt was to come up with a high-vol scheme. We decided that we should try to get it into a form that we would have a chance to use in the field. Our effort was aimed at adapting it to the high volume sampling system that we had for mass sampling at this low concentration. That train is commercially available from the Acurex people. Again, I prevailed on Joe [McCain] and Wally [Smith] to come up with some cyclones that would give us three cuts when operated under 5 cfm conditions, and they did (Figure 3). Unfortunately, I didn't stipulate in the contract that they had to carry it out on its first field test. All the cyclones operated fairly well, but the large cyclone was about 3 feet high. It did cut at 12 microns and we were aiming for 10, but it didn't fit in the Aerotherm oven. The basic idea still works very well. We got three different size classifications plus we had plenty of material for the toxicity people. We had a series of 10 tests, run up and down the West Coast by TRW Systems, using that scheme. Those poor boys did not love to lug this thing up and down on their backs, and I have heard about it every time that I have seen them since.

We did get sufficient sample to do some cytotoxicity testing. The preliminary results have given us enough shocks so that we decided to try to get a system that would be more useful. The data indicated that some of these plants that we have previously assumed to be innocuous in certain size ranges, usually predominantly in the respirable size range, weren't too nice at all.

The next iteration involved asking the Acurex people to try to get the cyclone train into a configuration that we could move around a little bit better. Figure 4 shows the scheme that they have come up with. They have the whole unit sitting inside the oven. For people familiar with their

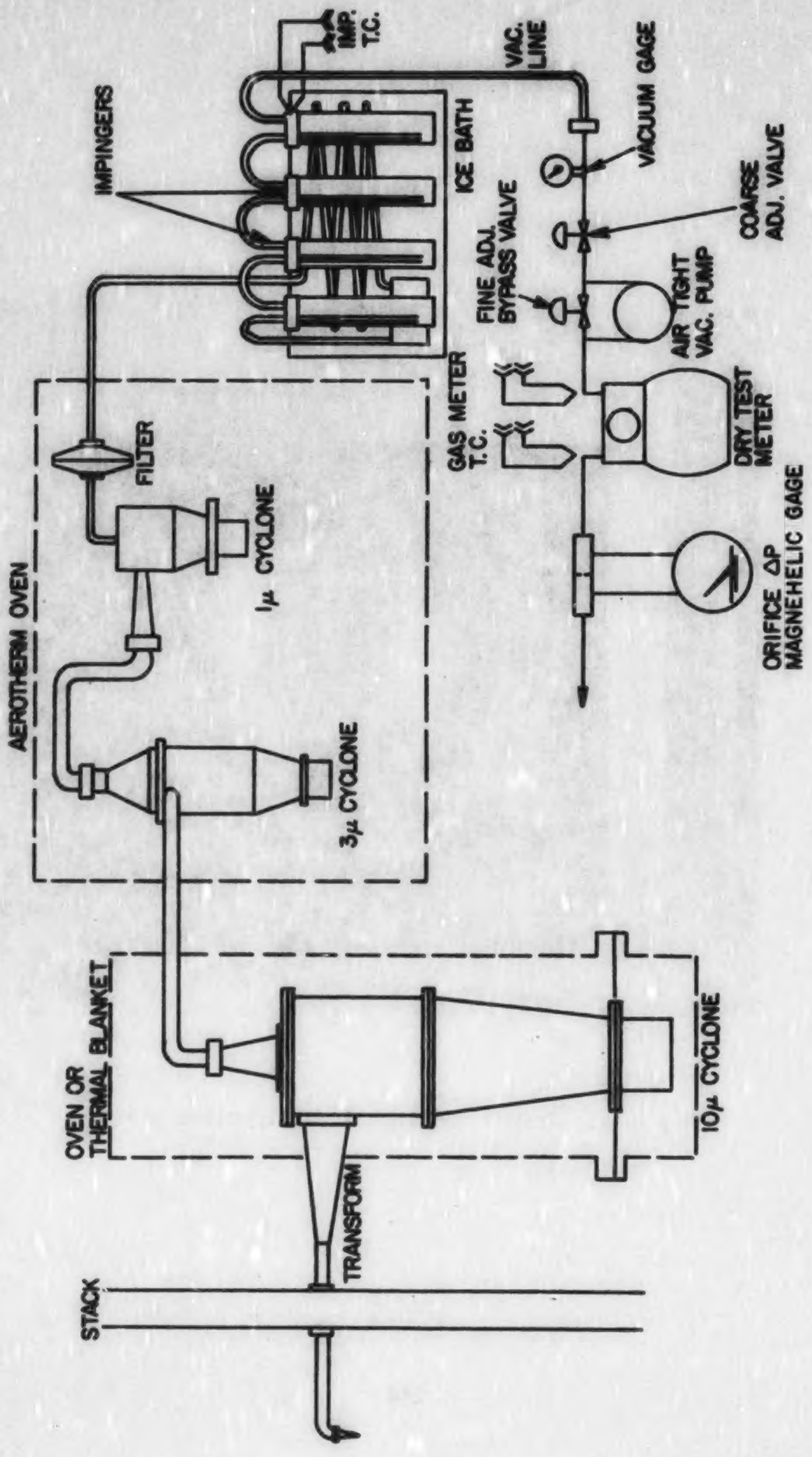

STACK
TRANSFORM
OVEN OR THERMAL BLANKET
10μ CYCLONE
AEROTHERM OVEN
3μ CYCLONE
1μ CYCLONE
FILTER
IMPINGERS
ICE BATH
IMP. T.C.
VAC. LINE
VACUUM GAGE
COARSE ADJ. VALVE
FINE ADJ. BYPASS VALVE
AIR TIGHT VAC. PUMP
GAS METER T.C.
DRY TEST METER
ORIFICE ΔP MAGNEHELIC GAGE

Figure 3. SoRI concept of multicyclone train.

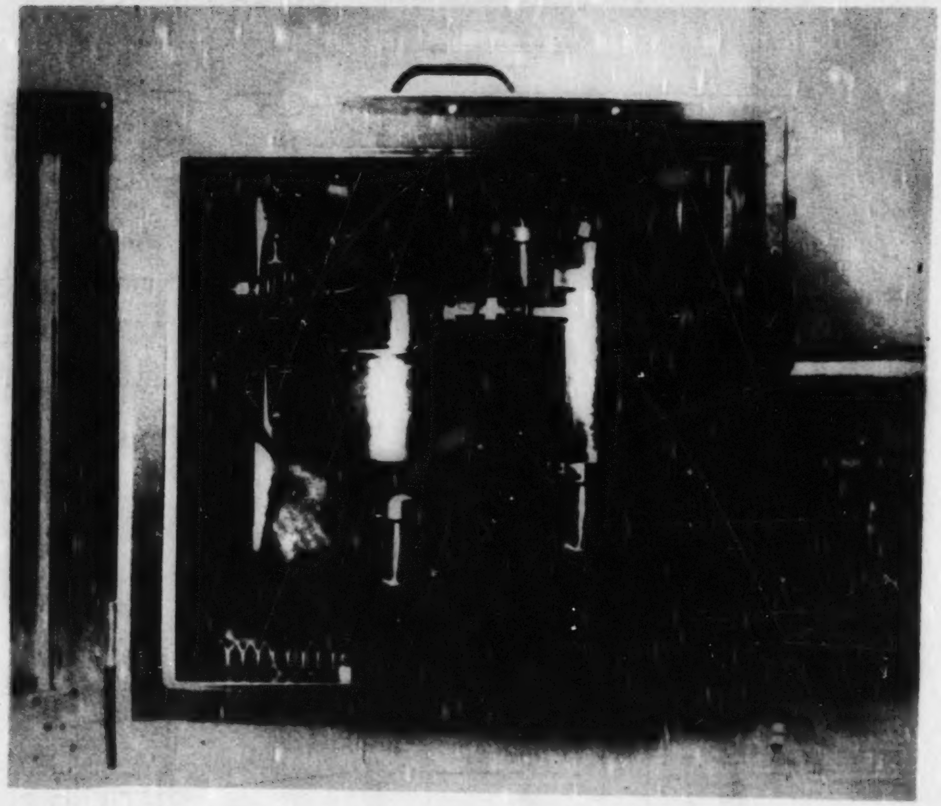


Figure 4. Acurex concept of multicyclone train.

normal unit, this one is about 3 inches bigger on each side than the standard. Something you can manage, but still fairly large. The main difference is that they used a stub-cyclone configuration similar to what we've seen with the Andersen and the Sierra impactor, as opposed to a classically designed cyclone that has been tapered. It has a separate collection box and it has the capability of using the pressure drop across one cyclone as the flow rate control.

Figure 5 shows the first calibration data we had on this unit. We were aiming at 10, 3, and 1 micron cuts. Anything larger than 10 microns we figured was going to hit somebody in the head rather than get inside the body, so we didn't worry about that too much. Three microns is the classical definition running around here (today, anyway) of what is respirable, and 1 micron was our feeling for the size range which would penetrate into the alveolar area of the bronchial system. However, we ended up with probably the most efficient cyclone the world has ever seen. The little unit was running about 0.3 micrometer for its D_{50}. The reason that this is extrapolated on the plot is that we ran out of capability to generate material small enough to calibrate it. So that curve is extrapolated and the shape below 1 micron is uncertain. The other cut points missed a little bit. We got 5 to 12 microns. Since we had this idea in our head that we wanted these certain size cuts, we asked Joe [McCain] to fiddle around a little bit and see what variations he could make. The results are shown in Figure 6. Basically by changing the inlet and outlet tubes, he came up with the type of configuration needed. The main reason I present this is to say that cyclones seem to lend themselves very well to tuning at a specific flow rate to whatever size range you are interested in. With the range we have in Figure 6, something like 1.5 to 4.5 microns, it does not seem to be out of the realm of possibility to think of making a unit with a number of stage cuts similar to an impactor. Impactor data and cyclone data seem to agree fairly well. For the data Bill [Kuykendal] presented this morning, the cyclone cuts were laying right over the curves that we were getting with impactors.

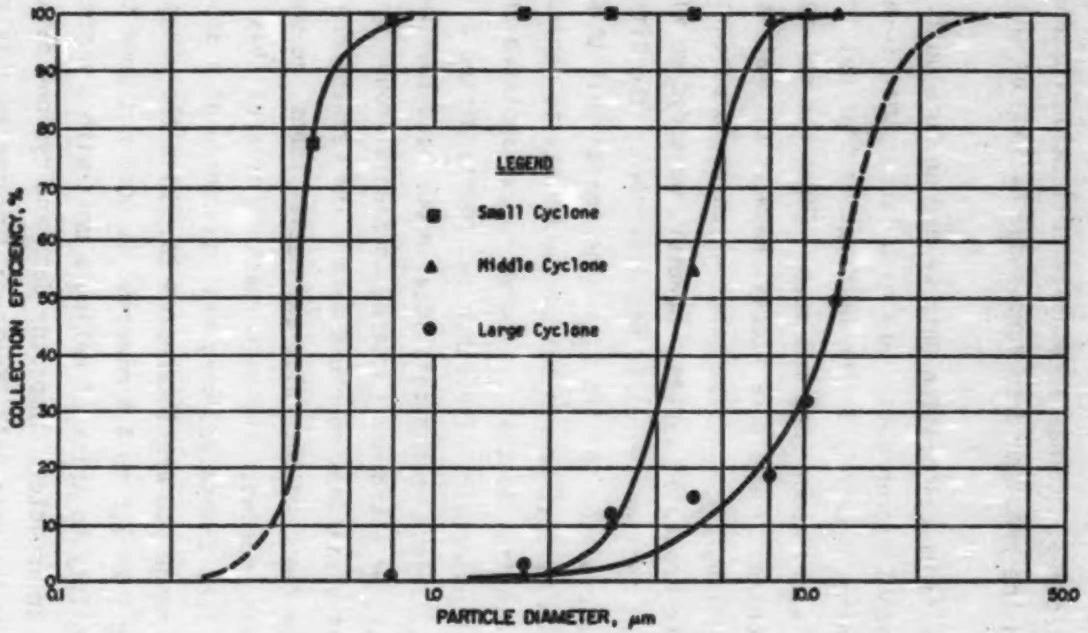


Figure 5. Collection efficiency versus particle size for the 5 ACFM Series Cyclone. (4 ACFM, 1.35 g/cc, 29.5" Hg, 22 °C).

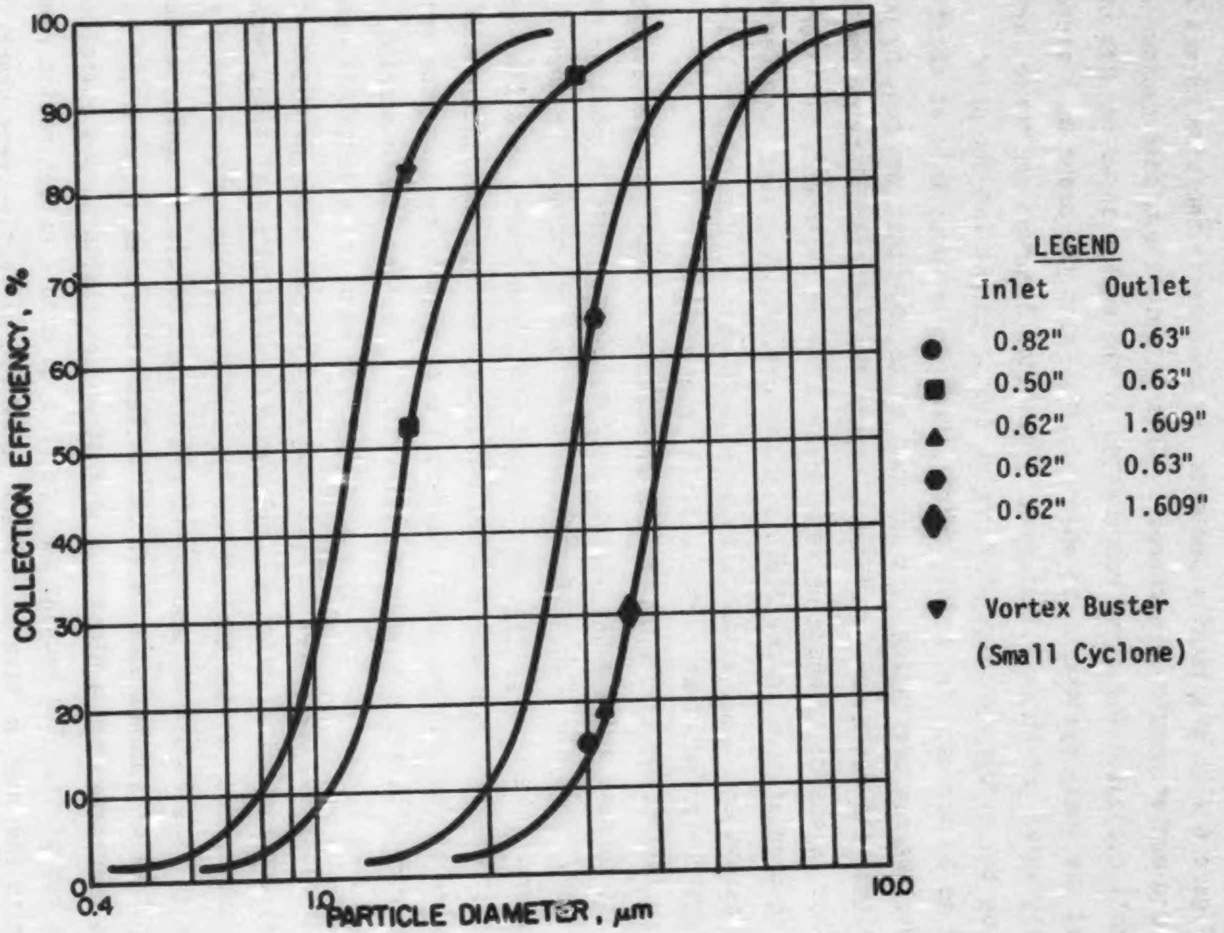


Figure 6. Collection efficiency versus particle size for the 5 ACFM Series Cyclone with modifications to middle and small cyclones. (4 ACFM, 1.35 g/cc, 29.5" Hg, 22 °C).

Figure 6 also graphically presents the changes in the size curves as inlet and outlet nozzles are changed. This is both the middle cyclone and the small cyclone. The curve represented by the square shows results of pushing the small cyclone up to where its D_{50} is on the order of 1 micron, actually we've got about 1.2 microns. The other 3 curves are three variations on the middle cyclones where we're trying to get the cut point closer to 3 microns. The middle of the three curves does this at about 3.5 microns. We are presenting this information to show that you can, by working with either the outlet or inlet tube, change the cut capability of the cyclone.

That is basically where we've come to with the cyclones. We'll probably be using more of them in the field since we are becoming more involved in the source assessment idea, which is to be able to push back to the health end of things the effect that particle sizing might have.

The last thing that I would like to present is something for everybody who wants to make a few bucks if they ever build it. This is what we need for an ideal particle size instrument as our needs stand now. Figure 7 shows the criteria. We need in situ measurement and furthermore, we'd like it so that at the time that we're doing the actual sizing, the stack doesn't know we're around. If you don't have sample nozzles to mess up your results, then your results are not going to get messed up by the sample nozzles. At the same time, after you get done sampling, you want to collect this sample. The size distribution should be kept separated for the other problems of chemical and biological testing. All the data should be real time, because of all the process upsets that people have told us about which cause problems with impactors. We need to get rid of that problem. Again, we have the problems of simultaneous inlet and outlet measurements. We might be able to feed data into a black box so that we get instantaneous efficiency results out of it. The machine must be able to handle concentration ranges similar to the ones we have seen where we've got perfectly clear stacks or ones we can walk across. Our size range needs and our temperatures (and we're in the process of moving the upper temperature up as we get into the coal conversion schemes where we have to face 1600 or 2000°F) are shown. So you can have a little flexibility here in the upper limits. All of this, of course, should be able to fit into something the size of an HP 65 so that everybody can carry it around with them. Any questions?

IN-SITU PARTICLE SIZING

COLLECTION OF SIZE FRACTIONATED SAMPLE

REAL TIME OUTPUT

SIMULTANEOUS INLET AND OUTLET MEASUREMENTS

PARTICULATE CONCENTRATION RANGE: 0.01 to 25 g/m^3
10 to 10^{10} particles/cc

AERODYNAMIC SIZE RANGE: 0.01 TO 10 MICROMETERS

TEMPERATURE: AMBIENT TO 450°C

PORTABLE OPERATION

Figure 7. "Ideal" instrument features.

DISCUSSION

_____: Bruce, when you show a point on your rate efficiency curve, is that the result of one run or the average of several runs?

HARRIS: It varies. On a calibration of the cyclone in the original report, I think that it was the average of several runs. At the point where we were trying to do the repair job, i.e., to alter the cut points, it was one run because we had problems to fix and we had one in the field and we had to get the data out in one or two days. This work was done for us by Southern.

_____: From back here, the cyclone looked awfully good. Could you tell us what the costs are?

HARRIS: Well, the first set of data was with the 3 foot high set of instruments. I don't know yet what it is going to be by the time we get all the ramifications that we want done in it, done. The cyclones themselves are on the order of $4 or $5K per modification if you just wanted to do that. If you wanted to get all the modifications we're having put into the unit in order to do the source assessment (which means we're having to insert an organic trapping device capable of trapping organics at a temperature somewhere between the oven temperature and the temperature of the impinger where we get the condensible inorganics), we're probably talking about a total package starting to approach $20,000.

_____: So the expense would keep you from building, let's say, a six-stage cyclone.

HARRIS: No, I don't think it would be that much.

MCCAIN: You can build a six-stage cyclone train for about the same price as you can build a six-stage impactor, maybe cheaper.

_____: Okay. Why wouldn't you do it then?

HARRIS: Well, for one thing you then expand the volume that you have to heat. Since this is an extractive system, you have to use an oven or some heating method. There are not too many people with 4 foot holes in their duct. If your conditions allow you to run at 1 cfm instead of 5 cfm and you get enough dust to work with, then you can get smaller. Another thing you could do would be to change to an axial type of configuration and use the higher flow rates.

OLIN: We're looking into designing something like this commercially. At first cut I would say that the price of 3 cyclone stages followed by a filter that fits into a 3 inch port (I think that that's an essential ingredient) would probably run you about the same price as a standard Cascade impactor.

_____: Well, the Cascade impactors run over a large price range. Would you give me a little better idea?

MCCAIN: $500 a cyclone.

OLIN: That sounds about right. I'd like some feedback on this because we fooled around with it a little bit in terms of preliminary designs. I'd say something that runs from say 0.2 cfm up to 1 or 2 cfm so that you can get the outlet of low or high efficiency devices and maybe 3 cuts. I think more than that does become fairly complicated.

HARRIS: The big problem that we have was the source assessment needs. We're going after 5 cfm.

_____: I always thought that the problem was that you couldn't get down on particle size, but you sound like you can get down to 0.3 of a micron.

HARRIS: At 5 cfm we do. We haven't tried to get down below that at 1 cfm. The one that we had originally set out for 0.5 micron ended up cutting at 0.8. The problem that we've had so far is that we don't have any equation which applies to cyclones, especially when you get down to small sizes. All we have is the classical cyclone equation. It seems that when you shrink the cyclone down to where you have a significant portion of the flow stream exposed to the wall, so that wall effects come in, the classical cyclone equation just goes to pot. We haven't come up with an equation that is able to describe what's going on. It's been a matter of something like: we've got this body and this head and we're going to change the inlet diameter, change the outlet tubes, put in a flow buster to break up the action a little bit.

OLIN: I think that the thing that is evident here from the work done by SRI and EPA is that you are in a different regime. It's a laminar regime and the classical turbulence equations of Lapel and others just don't seem to apply. Indeed, you can get very small cutoffs without reentrainment. So I think that it is a kind of new area. The one disadvantage is that you're not collecting the material on a filter or something, so you have to brush the particles out of the cyclone.

HARRIS: The thing that helps in that respect is that we're aiming to collect 0.5 or 1 gram or even greater quantities.

OLIN: To collect larger amounts.

RAO: One thing, if you plan to use cyclones with different flow rates, what calibration do you use?

HARRIS: Probably just use a function of the flow rate through the cyclone until we get something better. Get points at two different flow rates and extrapolate.

CONVERSION FACTORS

Although EPA's policy is to use metric units in its publications, certain nonmetric units were used in this paper for convenience. Readers more familiar with the metric system are asked to use the below factors to convert to that system:

cfm x 28.3 = liters/min
5/9 (F-32) = C
ft x 0.3 = m
in. x 2.5 = cm

FIELD EXPERIENCE WITH CASCADE IMPACTORS FOR BAGHOUSE EVALUATION

Reed Cass, GCA Corporation

SPARKS: It looks as if everyone is here, and most important of all our first speaker is here; we'll get started. We're going back to impactors now. The first speaker is Reed Cass of GCA and he is going to talk about how cascade impactors work when you're trying to evaluate a baghouse.

CASS: Gentlemen, GCA has recently conducted sampling programs on two utility boilers fitted with fabric filter dust collectors. In-stack impactors were used to measure the fractional particulate collection efficiencies of the baghouses. I would like to relate some of what we learned while using the impactors, focusing on the precision and accuracy of the impactor measurements and the problems which we encountered.

The first testing program was conducted at Nucla, Colorado generating station, which has been previously discussed. Steam is generated at Nucla by Springfield stoker-fired, traveling-grade boilers with fly ash reinjection These boilers are fitted with Wheelaborator-Frye fabric filter dust collectors, which use reverse flow and shaking for cleaning.

The second testing program was conducted at the Sunbury Steam Electric Station which is located in central Pennsylvania. The station has a capacity of approximately 400 megawatts, which are generated by four steam turbines. Two of the turbine units, rated at 87.5 megawatts each, are supplied by four anthracite fired Foster-Wheeler boilers. These boilers burn a pulverized fuel mixture of 15 to 35 percent petroleum coke with the remainder made up of anthracite silt and No. 5 buckwheat anthracite. Each of these boilers is served by a Western Precipitation fabric filter dust collector, which utilizes reverse air flow to clean the bags.

In the series of 22 tests at the Nucla station, the utilization of two identical Andersen impactors sampling simultaneously for 6 hours afforded an opportunity to examine the precision of the technique. Figure 1 shows the sampling locations in the stack for the impactors as well as

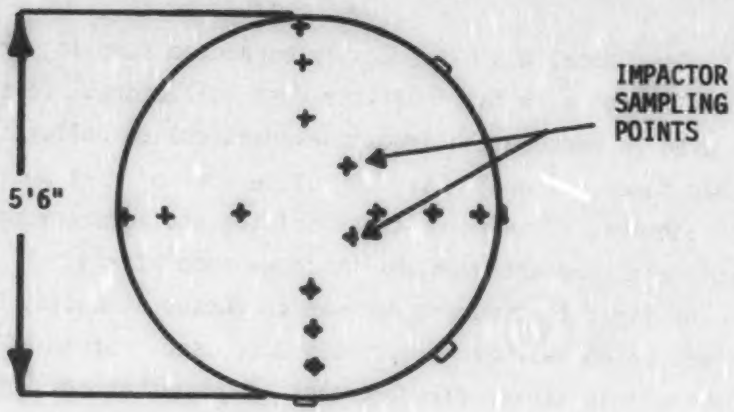

5'6"
IMPACTOR
SAMPLING
POINTS

Figure 1. Cross section of baghouse outlet sampling location showing sampling points.

the traverse points for an Aerotherm High Volume Sampler, which was used according to Method 5 technique. The stack cross-section was located more than 8 diameters from the nearest upstream and downstream disturbances, providing what should have been a homogeneous effluent for the impactors.

Table 1 presents the geometric mean mass concentrations as measured by each impactor and both impactors. As can be seen, the geometric mean concentrations as measured by each impactor are very close to each other. However, the average absolute value of the differences of each pair of measurements is about 70 percent of the geometric mean of all measurements. This means that for any given paired sample, a significant difference is quite apt to be observed between the two impactors. If one does not take the absolute value of the difference, however, the average difference is only about 18 percent of the goemetric mean of all of the measurements. This shows that one impactor does not always tend to be biased, and that the differences, though large, are probably random.

Table 2 presents an analysis of the mass median diameters as determined by each impactor. This shows the same trend as the mass concentrations analysis, except that the geometric standard deviations are somewhat smaller. Further, the average absolute value of the difference of each paired measurement is only about 40 percent of the average geometric mean of all the measurements; and the average difference between each pair, not taking the absolute value, is only about 5 percent of the geometric mean. It would appear, therefore, that substantial, apparently random differences are quite apt to be observed in the mass median diameters as determined by paired impactors, but that those differences are less than those observed for the measured mass concentrations. The accuracy of the impactor measurements was determined by comparing the geometric mean of the impactor concentrations with that of Method 5 outlet measurements. The Method 5 geometric mean concentration of 0.0025 grains per dry standard cubic foot is only about 66 percent of the impactor geometric mean concentration. Despite the poor agreement between the Method 5 and impactor measurements in terms of mean concentration, no specific difficulties were encountered and the actual field use of the impactors was nearly trouble free.

Table 1. COMPARISON OF OUTLET IMPACTOR CONCENTRATIONS

	Geometric mean concentration grains/dscf	Geometric standard deviation
Impactor X	0.0041	2.0085
Impactor Y	0.0036	2.2982
Impactor X + Y	0.0038	2.1419

$\frac{\Sigma|X - Y|}{n} = 0.0026$ $\frac{\Sigma(X - Y)}{n} = -0.0007$

Table 2. COMPARISON OF OUTLET IMPACTOR MASS MEDIAN DIAMETER

	Geometric mean mass median diameter, μm	Geometric standard deviation
Impactor X	8.57	1.51
Impactor Y	8.13	1.50
Impactor X + Y	8.34	1.50

$\frac{\Sigma|X - Y|}{n} = 3.26$ $\frac{\Sigma(X - Y)}{n} = 0.45$

At Sunbury, however, several problems with in-stack impactors arose, which not only severely complicated field use but also adversely affected the accuracy of the data. These problems included anomolous weight gains on the Andersen glass fiber substrates, weight losses of University of Washington greased impaction inserts, skewed size distributions due to excessive particulate capture while using an Andersen cyclone precollector and due to probe losses which amounted to significant portions of the total sample.

The anomolous weight gain problem was first indicated while sampling at Sunbury when it was noticed that the weight gains of the Andersen glass fiber substrates during the 6 hour test were much greater than would have been expected based on visual inspection. The substrates, which gained substantial amounts of weight, had almost no signs of particulate on them. To test the theory that the Andersen substrates were gaining weight which was not particulate, a particle-free sample flue gas was drawn through an impactor for the normal sampling time of 6 hours. As shown in Table 3, when the Andersen substrates were weighed, they had all gained weight, confirming that the weight gain was not due to particulate. In order to compensate for the anomolous weight gain, the Andersen impactors were loaded with two substrates on each stage with the sample stream impinging on the upper of the two substrates. It was hoped that subtraction of the nonparticulate weight gain of the lower substrate from the total weight gain of the upper substrate would compensate for the anomolous weight gain. To determine if the weight gains were the same for the upper and lower substrates, an impactor loaded with double substrates sampled particle free flue gas for 6 hours. Table 4 shows the weight gained by the upper and lower substrates for each stage. As can be seen, the lower substrate weight gains are approximately half of those of the upper substrates. The anomolous weight gain problem is believed to be the same as that which has been thoroughly studied by Messrs. Smith, Cusing, Lacey, and McCain of Southern Research Institute.

A second approach to the anomolous weight gain problem was to sample with the University of Washington Mark III impactor which uses stainless steel

Table 3. ANDERSEN SUBSTRATE WEIGHT GAINS WHEN SAMPLING FILTERED FLUE GAS

Stage	Weight gain, mg
Prefilter	5.7
0	4.8
1	3.5
2	3.8
3	2.6
4	2.0
5	1.8
6	1.6
7	0.1
F	0.8

Table 4. ANDERSEN SUBSTRATE WEIGHT GAINS WHEN SAMPLING FILTER FLUE GAS

	Substrate weight gain, mg			
Stage	Upper	Lower	Upper-lower	$\frac{\text{lower}}{\text{upper}} \times 100$
Prefilter	4.5			
0	6.3	3.3	3.0	52
1	6.4	2.9	3.5	45
2	5.2	1.8	3.4	35
3	4.3	2.0	2.3	46
4	2.1	1.0	1.1	48
5	1.7	0.8	0.9	47
6	1.2	0.5	0.7	42
7	1.0	0.6	0.4	60
F	0.0	0.3	-0.3	

inserts coated with grease as impaction surfaces. Initially, the inserts were coated with Dow Corning Hi-Vacuum grease, however, when these inserts were weighed after sampling, it was found that they had all lost weight. After some experimentation, inserts coated with polyethyleneglycol and dried in an oven at 300°F overnight were found to be satisfactory, showing substantially decreased weight loss problems.

In essence, we found substantial weight gains for the glass fiber substrates, but we were able to compensate for them at least partially. We also found the losses using Dow Corning Hi-Vacuum Grease to be too large for our purposes, but polyethyleneglycol coatings performed satisfactorily, although also producing some losses.

ENSOR: What was your change in weights with the glycol? Do you have the values?

CASS: No sir, I don't.

When the size distribution curves for the first 21 Sunbury inlet Andersen impactor tests with a cyclone precollector were plotted, the size distribution curves indicated that the precollector was collecting a large portion of particles which would have normally been impacted on the upper impactor stages. The curves shown in Figure 2 are clean examples of how the form of the curves is affected by the precollector. In an attempt to determine what portion of the particles was being removed from the upper stages by the precollector, an additional impactor sample was taken through a gooseneck probe for each of the remaining 10 tests.

Upon examination of cumulative distribution curves for these runs, no consistent difference between the size distribution determined by the Andersen with the precollector and the Andersen with the gooseneck nozzle could be found. This is apparent from the size distribution curves presented in Figure 3. Also the mean percentage of the mass collected by the impactor precollector for the tests was approximately 35 percent of the total mass collected while the mean percentage of the mass collected in the gooseneck probe for the same series of tests was approximately 33 percent of the total mass collected. Thus, it appears that both the cyclone precollector

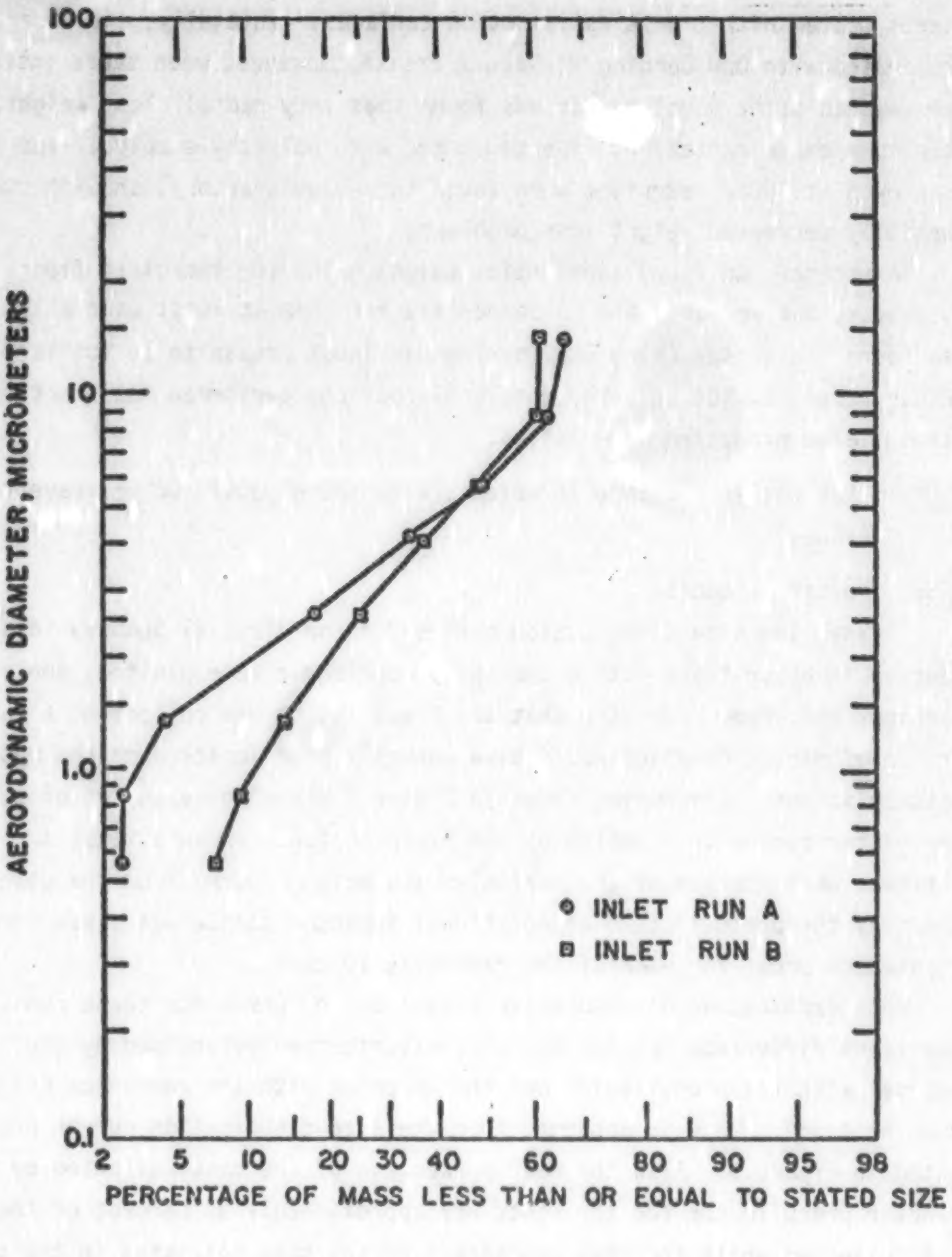


Figure 2. Inlet cumulative particle size distribution for run 17.

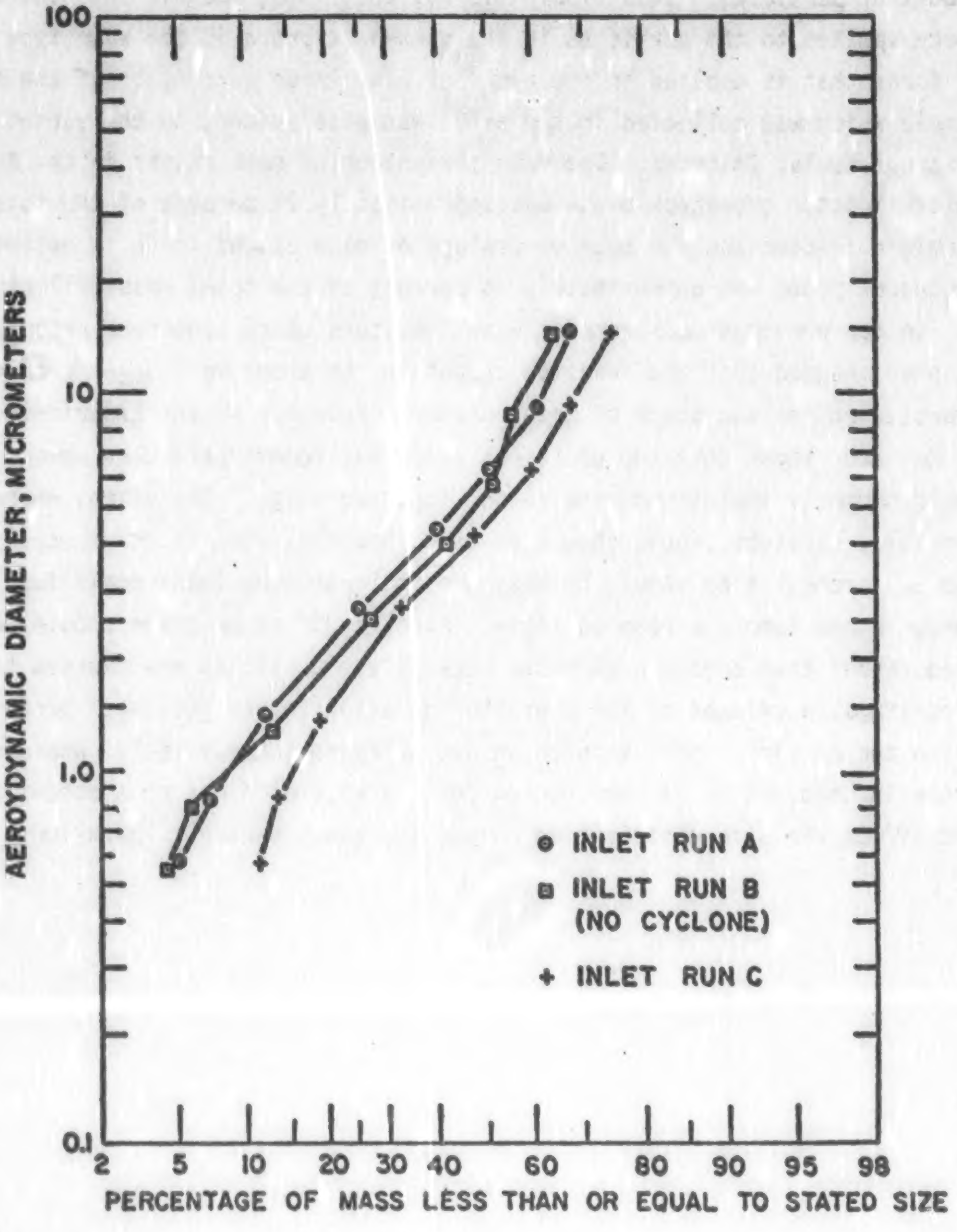


Figure 3. Inlet cumulative particle size distribution for run 23.

and the gooseneck probe removed approximately the same size fraction and amount of particles. This finding is not surprising because the centrifugal force applied to the particles in the gooseneck probe is the same type of force that is applied in the cyclone. The large percentage of the total sample which was collected in the probe was also evident in the series of tests at Nucla, Colorado. The mean percentage of mass caught in the Nucla inlet impactor gooseneck probe was approximately 21 percent of the total sample collected and the mean percentage of mass caught in Nucla outlet gooseneck probe was approximately 15 percent of the total mass collected.

In our previous measurements with impactors using gooseneck probes, we have assumed that the fraction caught in the probe was larger than that impacted on the top stage of the impactor. From our recent experience, it has been shown that the gooseneck probe may remove particles which would normally impact upon the second impactor stage. Therefore, whenever possible, straight probes should be used; however, when it is necessary to use 90° probes, they should be designed to impart the least contrifugal force to the sample stream possible. Also, a 90° elbow probe should be used rather than a gooseneck probe because the particles are exposed to centrifugal acceleration for a greater duration in the gooseneck probe in which the particles pass through an arc of approximately 160°. Whatever probe is used, it should be checked for losses, which may be substantial and affect the size distribution curves for the impactor. Thank you.

DISCUSSION

ENSOR: When you noticed the weight gain on the substrates, did you ever run two of the substrates through the filtered flue gas to see if they gained the same amount of weights?

CASS: Yes, we did. That is presented in Table 4.

_____: Was that what that table was? The extra weight was the particulate?

CASS: No, that was when filtered flue gas was passed through the impactor.

SPARKS: Thank you.

LOW PRESSURE IMPACTORS FOR IN-STACK PARTICLE SIZING

Dr. M. J. Pilat, University of Washington

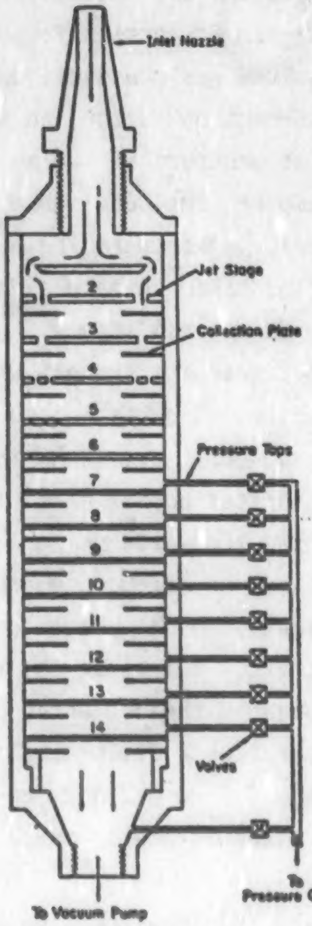
SPARKS: About everybody is back so we will get started again. Awhile back we got interested in defining the performance of particulate control devices in the particle size range below which conventional cascade impactors work (from say 0.1 to 0.01 micron diameter). The next two people are going to tell us about a couple of techniques for measuring fine particles, and then this afternoon we will have a panel discussion on another method.

Mike Pilat is our first speaker, and Mike has been working on a super-high-pressure drop cascade impactor to push the sizing capabilities down below 0.1 micron diameter, and he is going to tell us all about how good this impactor is.

PILAT: To provide a proper perspective for our type of research at the University of Washington, I think we should be classified more as users of cascade impactors in support of our research on plume opacity, source emission characterization, and particulate control equipment development.

After our initial work on the Mark I, II, and III models of our U of W Source Test Cascade Impactors, which size particles in the 0.3 to 30 micron diameter range (and where the now commercially available cascade impactors operate), we have focused our cascade impactor research on those particles in the 0.02 to 0.2 micron diameter range. This research has resulted in a cascade impactor with gas pressure taps located on those impactor stages with less than 0.2 micron D_{50} stage cut diameters, as shown in Figure 1. These pressure taps are to monitor the absolute gas pressure in order to obtain the magnitude of the Cunningham Correction factor, as shown in equation 1, used in the design of cascade impactors,

$$D_{50} = \left(\frac{2.61\ \mu\ D_j}{C\ \rho_p\ V_j}\right)^{1/2} \qquad (1)$$


Inlet Nozzle
Jet Stage
Collection Plate
Pressure Taps
Valves
To Pressure Gauge
To Vacuum Pump
2
3
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13
14

Figure 1. Cross section of prototype Mark IV U.W. Source Test Cascade Impactor.

assuming 0.145 for the Stokes number and with μ the gas viscosity, D_j the jet diameter, ρ_p the particle density, and V_j the gas velocity in the jet. As a matter of practicality, the jet diameter is limited to about no smaller than 0.008 inch diameter, the gas viscosity is fixed, the particle density is set, the gas velocity is probably limited by sonic flow, but the Cunningham Correction factor is not limited. Therefore, by reducing the absolute gas pressure, one can get larger magnitudes of the Cunningham Correction factor, as shown in Figure 2. For example, at an absolute gas pressure of 100 mm Hg, unit density, and 0.02 microns particle diameter, the Cunningham Correction factor is larger than 100. Therefore, in essence, the Mark IV model is a cascade impactor in which we have a high pressure drop and low gas absolute pressures on the downstream stages.

In 1971-1972, we had a graduate student who performed a laboratory study on our Mark IV impactor. A potassium sulfate aerosol was generated using a modified Dautreband nebulizer. Scanning electron micrographs were taken of the particles deposited on the particle collection plates. These particle images were counted and sized using a Zeiss Particle Analyzer. When using the scanning electron microscope procedure it is necessary to be careful with the application of an electrically conductive layer (gold, palladium, carbon, etc.) such that you neither cover up nor significantly alter the size of the collected particles. After the laboratory study, we constructed a field sampling model (Mark IVC) which operates at about the 0.2 to 0.3 acfm gas flow rate. The Mark IVC sizes in the 0.02 to 0.2 micron diameter range and is located downstream of the Mark III.

Using the Mark III and IVC impactors, we measured particle sizes at a sulfite pulp mill and a pulverized coal-fired boiler. Based on the results of these tests we designed a Mark IVD model which is mainly oriented towards sampling at the outlet of control devices where the particle mass concentration is lower. The Mark IVD samples at gas flow rates up to 1.8 acfm. The Mark IVD sampling train was improved to include a control unit to house the pressure gauges and temperature controls, a low pressure drop tubular condenser, and a larger vacuum pump. A 90-mm diameter filter

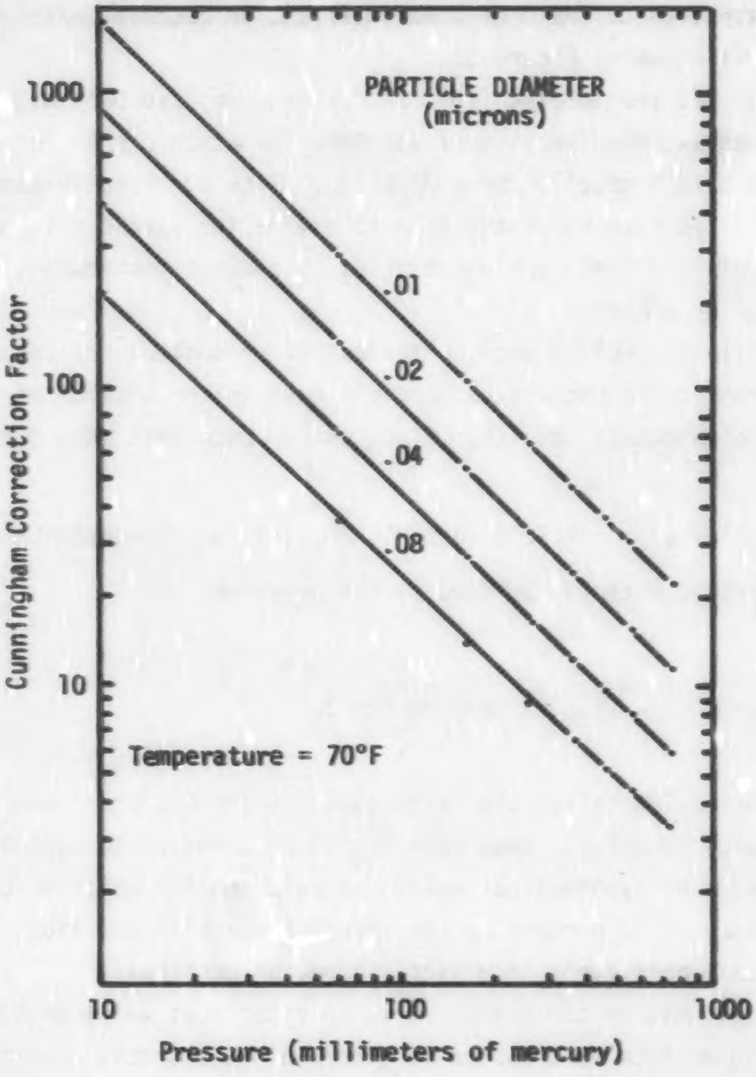

Figure 2. Cunningham Correction Factor as a function of absolute gas pressure.

holder was used (instead of the 47-mm diameter holder as is used in the Mark IVC train) in order to reduce the gas pressure drop downstream of the impactors. It is interesting to note that in some source tests we have been collecting very little particle weight in the filter, indicating that the particles are not bouncing on through the Mark IV even though we are using quite high gas velocities in the stage jets. A schematic illustration of the U of W Mark III and IV cascade impactor sampling train is shown in Figure 3.

For sampling at the inlet to control devices, we have been using a BCURA cyclone, an extended version of the Mark III which has 11 jet stages (we call this a Mark V model), and a Mark IVC. With the greater number of jet stages in the Mark V, we are able to divide the particle into a greater number of size fractions which helps to prevent overloading of the particle collection plates.

Our Mark III-IV sampling data at the outlet of control devices on coal-fired power plants has shown little particle mass in the smaller particle size ranges, approximately less than 2 percent by mass less than 0.2 micron in diameter.

CALVERT: What kind of particle diameter? Physical or aerodynamic?

PILAT: The particle diameter defined by the equation

$$D_{50} = \left(\frac{2.61\ \mu\ D_j\ N}{C\ \rho_p\ V_j} \right)^{1/2} \qquad (2)$$

which is conventionally called the "effective cut-off diameter" and using a particle density of unity. Thus this D_{50} is an aerodynamic cut-off diameter (diameter of hypothetical sphere of unit density which is collected with an efficiency of 50 percent by the impactor stage in question, regardless of the actual particle's true size, shape, or density).

At any rate, this is the status we're in right now; we're mainly heading towards that higher flow rate system in order to get a sample downstream in the same time increment as the upstream sample.

I have these handouts and I also have a paper that we presented at Vancouver, and I'll make that available to those who are interested, and I think that that's about all I care to say unless you have a few questions.

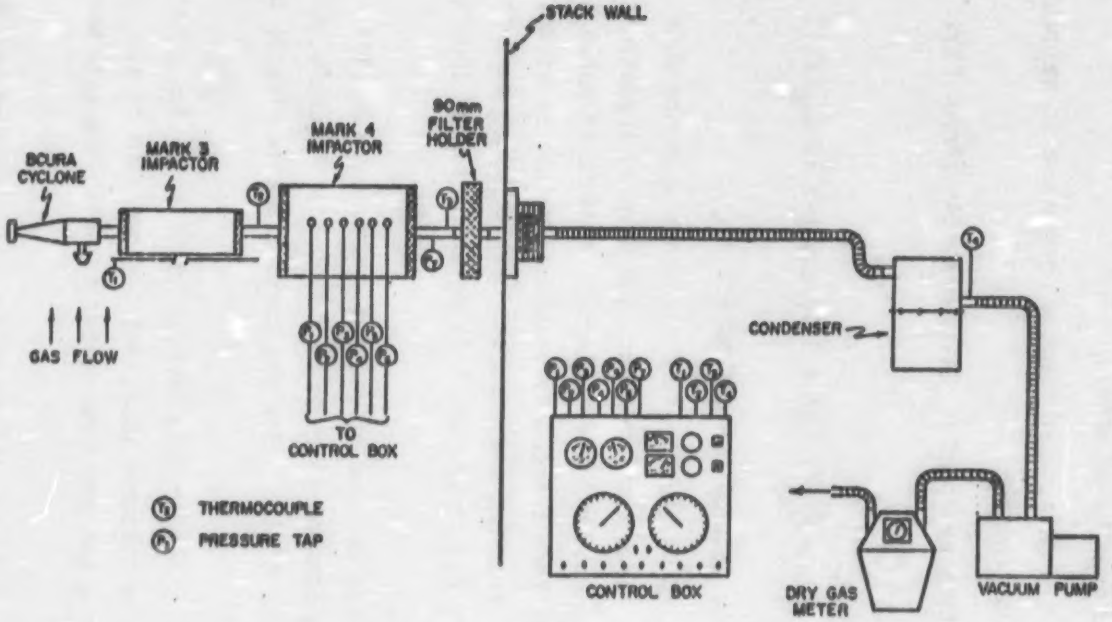
STACK WALL
BCURA CYCLONE
MARK 3 IMPACTOR
MARK 4 IMPACTOR
90mm FILTER HOLDER
GAS FLOW
TO CONTROL BOX
CONDENSER
THERMOCOUPLE
PRESSURE TAP
CONTROL BOX
DRY GAS METER
VACUUM PUMP

Figure 3. U of W Mark III - Mark IV impactor sampling train.

DISCUSSION

BOLL: Do your particle diameters assume a particle density?

PILAT: Yes, a particle density of unity.

BOLL: When you reduce the data, what formula do you use to calculate the Cunningham Correction factor?

PILAT: We use an equation reported by Davies in 1945 (Proc. Phys. Soc., Vol. 57, pp 259-270):

$$C = 1 + \frac{2\lambda}{D_{50}}[1.257 + 0.40 \exp (-1.10\ D_{50}/2\lambda)]$$

where λ is the mean free path of the gas molecules.

BOLL: The reason I asked is that I understand that when the Cunningham Correction factor gets large and important, you have to pay attention to how the gas molecules bounce off the particles, because it makes a difference in the slip. Sometimes they bounce off elastically, and sometimes they don't, and that's not important when you are talking about correction factors of 1.1 and 1.2; but if you are talking about 100 or 120, then it may be important.

NICHOLS: But that 100 is because of reduced pressure. You still have the same differential mass between your particle and your molecule, as if it were a correction factor of 1.1. You get the difference because of the vast change in pressure.

ENSOR: I think that a point to note here is that the Davies equation for C is an empirical fit to measured data, and the question is whether the equation is being used for a gas pressure region in which the data were originally obtained.

BOLL: But it changes with what the particles are. You are not going to mix water and oil particles.

PILAT: Perhaps I should discuss some of the possible problems. In the draft impactor guidelines which Bruce Harris handed out, the maximum gas velocity is in the 70 meter/second range. Well, we are exceeding that with gas velocities up to 0.9 Mach number. During our initial laboratory tests, we did blast-off the deposited particles on some stages of the Mark IV. However, we have modified the impactor stage design to correct this situation. The proof of these improvements is provided by the scanning electron micrographs of the particles collected during the field testing.

Another possible problem is the use of a Stokes number magnitude of 0.145 for the calculation of the particle D_{50}. Our studies have shown that this Stokes number at 50 percent particle collection efficiency of a stage is somewhat dependent on the jet Reynolds number and/or the absolute gas pressure. Our laboratory and field calibration work will clarify this situation. I hope this is not evading your question.

BOLL: No, I think it is to the point. But I do think one has to worry about whether the Cunningham Correction factor is the same for each particle material.

RAO: The effect of material is very little. It has been pointed out by Fuchs that we all use oil drop constants for everything.

BOLL: That is not my understanding.

CALVERT: Lee Byers knows all about that. He studied accommodation coefficients with regard to thermophoresis. I don't know whether the mystery got cleared up. It's been years.

BYERS: We were working at atmospheric pressure but at high temperature.

OLIN: Mike, when you drop the pressure, do you have a sonic orifice between the Mark III and the Mark IV?

PILAT: No, we stage down the gas pressure progressively. It is someone else who has the system that passes the gas through a throttling valve.

OLIN: Drop the gas pressure down and the whole impactor runs at a lower pressure.

PILAT: This other system is for sampling the atmospheric aerosol.

OLIN: Does that mean that the last stage on your Mark IV is running choked?

PILAT: No, not knowingly. We may achieve sonic flow sometimes because we are pushing some test prototypes at higher gas flow rates than they were originally designed for, mainly because we want to sample at the outlet of control devices which have fairly low particle mass concentrations. But our design is not such that we can go through a throttling setup to lower the gas pressure substantially in one stage at say Mach one. In fact, we are trying to even out the Mach numbers on the stages such that they are relatively constant.

OLIN: That is a little different approach that you are using compared to the standard low-pressure impactor.

PILAT: Well, it is mainly that we are trying to avoid possible problems that may arise at higher gas velocities such as the sonic shock wave at Mach one. It looks like, from the experimental data, the high gas velocity may not be that important and this is based on the fact that we are not seeing particles of too great a diameter (larger than the upstream stage D_{100}) in the filter and in the lower Mark IV stages with field samples. These particles are depositing on the proper stage collection plates and this implies that the particles are not bouncing or being blown off.

OLIN: So the Mark IV then is like your Mark III, but it has smaller holes and less of them. Is that it? So you are just going to higher and higher gas velocities?

PILAT: The main thing that is different on our Mark IV is that it has pressure taps on the stages, operates at a lower absolute gas pressure, and has a sequence of stage jet diameters and number of jets per stage that allows the gas pressure lowering to be achieved without having the gas velocity go sonic on any one stage.

OLIN: But the Mark IV operates at a lower pressure because you are dropping the pressure across succeedingly smaller and smaller orifices.

PILAT: Yes, now if you were to take the conventional commercially available cascade impactor and just lower the gas pressure at the impactor outlet by using a large vacuum pump, you can lower the stage particle D_{50}s somewhat. Our first prototypes of the Mark IV were longer bodied Mark III models with some pressure taps on the lower stages to watch what was going on.

CALVERT: I am not straight on the diameters. The diameters you reported, are these the computed physical diameters?

PILAT: Yes.

CALVERT: Isn't it the aerodynamic diameter?

PILAT: Well, it is the same aerodynamic diameter as reported by the other cascade impactors when calculated using the equation presented earlier.

CALVERT: What I call aerodynamic diameter is the diameter multiplied by the square root of the density and square root of the Cunningham Correction factor.

PILAT: I am referring to the stage D_{50}.

OLIN: For an equivalent spherical particle with density of one.

LILIENFELD: I have two questions. The first is what was the pressure at the last stage?

PILAT: This has varied over a considerable range, from say 3 to 10 inches of mercury. This range is dependent upon the Mark IV stage configuration (number of stages, jet diameter, and number of jet holes per stage) and upon the capabilities of the vacuum pump used.

LILIENFELD: The second question is what kind of substrates are you using?

PILAT: Excellent question. We have used flat foils (non coated foils) and greased substrates. In the Mark IV at higher gas temperatures, there is a problem with the weight loss of the grease. We have done quite a bit of testing with no grease at all. The scanning electron micrographs of these samples without grease looked quite good with regard to not having particles larger than expected on the Mark IV foils.

RAO: What about wall losses?

PILAT: I don't have the numbers here, but our test procedure frequently includes washing all of the parts using an ultrasonic cleaner and then evaporating the solvent to dryness and then weighing the residue. We have some disagreement between our field data and that laboratory data reported by SRI. We have not been able to find the nozzle losses which Wally did. In fact, we were strongly considering changing our nozzle design, but decided against it after looking at our field data.

SMITH: One thing that we did not do during our laboratory tests is to load up the impactors. That makes a difference.

PILAT: Yes.

SMITH: You are talking about milligrams which are pretty large quantities of material.

PILAT: The percentage particle losses appear not to be that great at all.

SMITH: With regard to coating surfaces and so forth, I think it is hard to generalize effectively the ultimate jet velocity. This is definitely a function of the particle size too. In the Sierra impactor, for example, the jet velocity was increasing with decreasing particle size, but the particle bounce was decreasing at the same time as you went to the smaller particle sizes. So who knows what happens when you go to the submicron particle sizes.

PILAT: Yes, in fact we have been working on coal-fired power boilers (research supported by the Electric Power Research Institute) and it appears that our main particle overloading (and then blow-off) problems may be in the 4 to 6 micron diameter range, not in the Mark IV size range.

OLIN: Well, while we are dwelling on this subject of bounce, maybe this should be held for discussion, but it seems to me that as you go to smaller and smaller particles, that the bounce effect does get smaller and smaller because it has to be something to do with the particle momentum which has the particle diameter cubed in it. It is particle mass times velocity so that the effect goes down like the cube of the particle size. Furthermore, the Van der Waals active forces zoom up for the smaller particles and thus, there are a lot of things going for you as you go to smaller and smaller sizes, which eliminates the problems of bounce. My feeling is that above 3 microns, you have bounce and below that I'm not sure.

PILAT: One of our graduate students designed a prototype Mark IV with very sharp cut-off curves and measured the collection efficiency versus particle size. His data showed that the larger particles were very effectively scoured off the particle collection plates. This illustrates the fact that it is possible to design and build a low pressure cascade impactor that does have the particle bounce and/or scouring problems.

_____: Mike, when you were using your impactor train in the field, did you have both the Mark III and Mark IV plates greased, or just one?

PILAT: At times we actually had both impactors uncoated. Sometimes we had grease only on the Mark III collection plates. We have not yet operated a simultaneous test with one train using greased foils and the other train having uncoated inserts.

_____: Mike, I understand you to say that you intend to calibrate at the small particle sizes. Could you describe this?

PILAT: We are working on generating an electrically conductive submicron aerosol so that we will not have to coat the particles with gold, graphite, etc. We have a gold hot-wire aerosol generator constructed and we are running tests with it. We will use scanning electron microscopy to size the particle deposits.

_____: How do you weigh the particles collected with your low pressure impactor?

PILAT: We use a Mettler balance. It is a real problem to achieve accurate weighing with the small samples. We have not been successful in using the Cahn electrobalance, either the model C2 or the model 4100, and we tried quite a bit on this.

WILLIAMS: What type of weights were you trying to weigh?

PILAT: We like, of course, to get samples in say, the milligram range. But sometimes we have low weights down in the 0.1 to 0.01 milligram range. You can sometimes have a visible particle sample that is in the noise range of the balance.

SPARKS: Thank you, Mike.

SUBMICRON PARTICLE SIZING EXPERIENCE ON A SMOKE STACK USING THE ELECTRICAL AEROSOL SIZE ANALYZER

Gilmore J. Sem, Thermo-Systems, Inc.

SPARKS: Gil Sem from Thermo-Systems will now talk about the field use of the mobility analyzer for obtaining size information down to below a few tenths of a micron.

SEM: The items I plan to cover today are:

1. A brief review of the design and calibration of the electrical areosol size analyzer,
2. A report on the successful use of the analyzer to measure the penetration of submicron particles through a baghouse filter on a coal-fired power plant, and
3. A discussion of problems we observed during the field program and suggestions for further work to solve these problems.

WHY SHOULD WE MEASURE SUBMICRON PARTICLES IN STACKS?

Although particles smaller than 1 μm diameter are only a small fraction of the total mass emissions of particles from coal-fired power plants, these particles are a very important component of the air pollutants emitted from the stack. Submicron particles remain airborne in the atmosphere for periods of days, traveling long distances from the source. When inhaled, these particles penetrate deeply into the human respiratory system, posing a more serious health hazard than the larger particles. The potential harmfulness of submicron particles, and their penetration through control equipment, is a function of particle size. Thus, measurement of the particle concentration as a function of size is important for characterizing the performance of control equipment as well as for characterizing the potential harmfulness of the emissions.

THE ELECTRICAL AEROSOL SIZE ANALYZER

The electrical analyzer, either in its present form or in the earlier, much bulkier form, has been used for over 10 years for the measurement of ambient urban air, smog chamber, and laboratory aerosols. It's only within the past year, or perhaps, past 6 months, that it has been applied to stack aerosols. Southern Research Institute, under Wallace Smith and Joe

McCain, has used it in several types of stacks, mostly within the last 3 months. Battelle Institute in Columbus has used it within the past 3 months for coal-fired power plant evaluation. Monsanto Research Corp. is preparing to use an analyzer for control equipment evaluation. Meteorology Research Inc., under David Ensor and Richard Hooper, is using it for baghouse evaluation on a coal-fired boiler. I believe one or two other groups are in the process of applying the analyzer on gas turbine engines.

Figure 1 shows the analyzer, consisting of 2 modules: a larger flow module (35 x 30 x 62 cm LWH) and a smaller control and readout module. The entire instrument weighs 20 kg excluding vacuum pump. The instrument basically places a known, reproducible electrostatic charge on each particle; a second stage removes particles smaller than a known, selectable size from the aerosol stream; a third stage measures the charge level carried by particles passing through the second stage. The measured charge level of the third stage is proportional to aerosol concentration. I will not discuss the theory any further in this talk.

Aerosol enters the center tube shown in Figure 1 at 4 LPM. An additional 46 LPM enters the instrument to serve as sheath air. The two streams do not need to come from the same source, but should be within several degrees of the same temperature and neither should be saturated with water vapor.

An analyzer has been calibrated by exposing it to a series of monodisperse aerosols of known sizes. All analyzers are compared with a calibrated standard instrument before shipment. If they are kept clean by proper cleaning every few weeks of operation, the calibration remains constant. However, it is best to compare a field unit with a laboratory-based instrument or with a known test aerosol before any critical program. If this precaution is followed, almost any field malfunction will be obvious from the data. Incidentally, I strongly recommend that anyone on a field program bring along a spare charger assembly. The charger is the most "breakable" item on the analyzer and can be field repaired only by replacement.

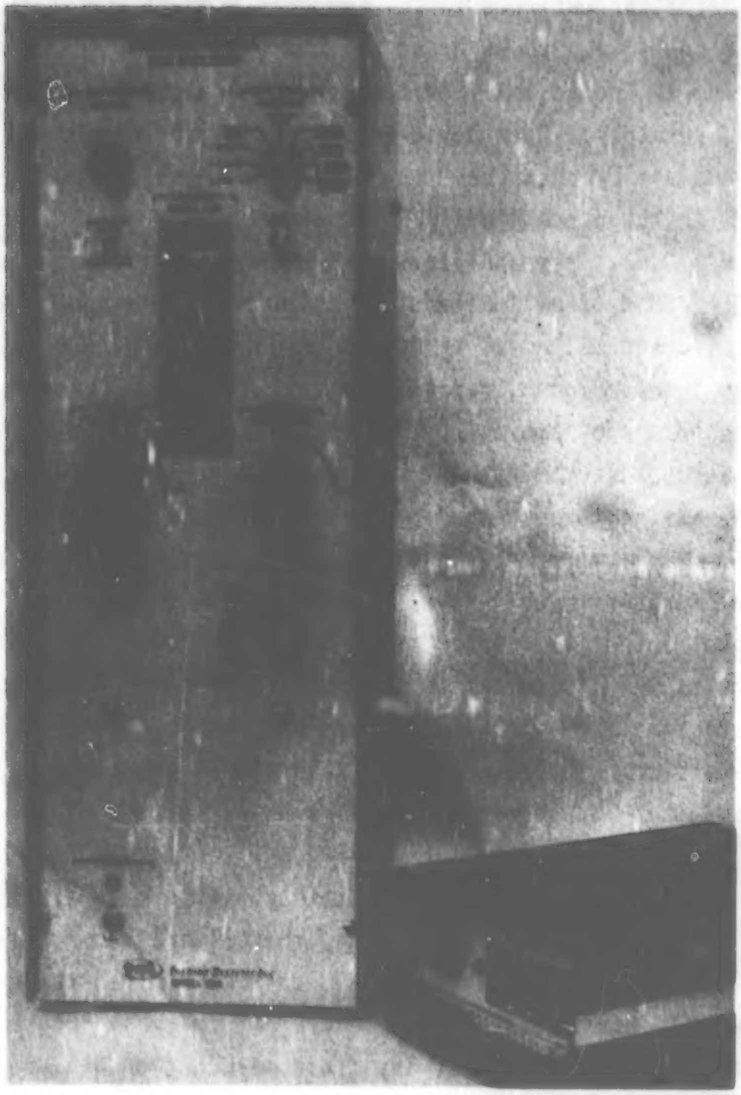

Figure 1. Photograph of the Model 3030 electrical aerosol size analyzer.

FIELD APPLICATION TO STACKS: NUCLA POWER PLANT

Figure 2 shows the power plant near Nucla. It is small, only 36 MW total; 12 MW in each of 3 boilers. The coal is mined about 6 to 8 miles away, and its composition is quite variable.

Figure 2. Nucla Power Plant of the Colorado-Ute Electric Association.

We measured the No. 2 boiler emissions. We had an excellent set of sampling ports on the downstream side of the baghouse. It was more than 10 diameters downstream of the last bend at the bottom of the stack. We had sufficient space for equipment.

The effluent from the stack was not visible at any time. The visibility as measured by an MRI stack nephelometer was in the range of 22 km inside the stack downstream of the baghouse. This was verified by the electrical analyzer which measured submicron particle concentration levels similar to those measured in clean rural outdoor areas.

Our equipment, shown at the downstream location in Figure 3, included a diffusion battery of parallel plate design, a Gardner nuclei counter, an electrical analyzer and (not shown) a diffusion drying system. The downstream sample coming from the stack was heated to about 160°F up to the entrance of the first diffusion drier. The aerosol than passed through about 3 to 4 feet of polyethylene tubing to a second diffusion drier before

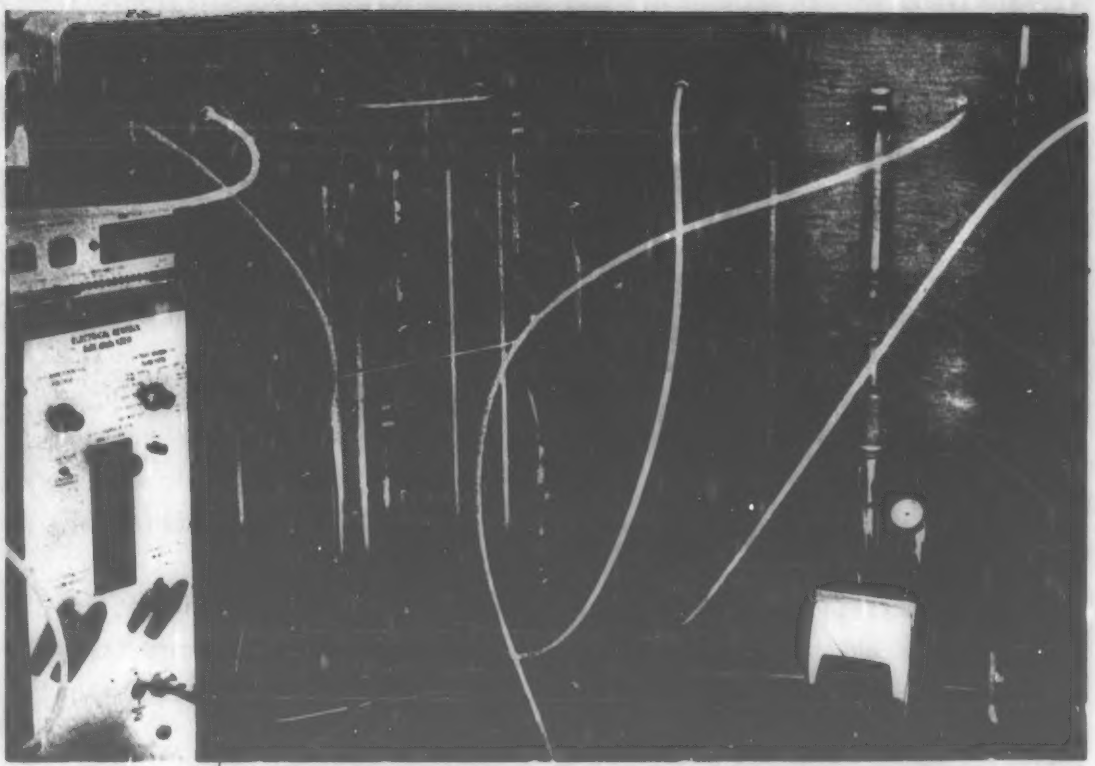

Figure 3. Instrumental setup downstream of the baghouse showing the EASA, the diffusion battery, and the Gardner nuclei counter.

entering the electrical analyzer and diffusion battery. The analyzer sheath air was dried before it entered the analyzer. Filter samples were taken behind each diffusion battery stage in addition to Gardner data. On some runs, the analyzer sampled behind each diffusion battery stage in an attempt to get one more field comparison of the analyzer and diffusion battery. I have no diffusion battery or filter data to show at this time; Ensor and Hooper will report fully at a later time.

The equipment, shown at the upstream location in Figure 4 without the diffusion battery, included a dilution system capable of ratios at least as great as 1000X. The emissions sample, heated to 160 to 220°F, enters a diffusion drier, then passes through the diluter, and finally another diffusion drier before entering the analyzer and/or diffusion battery. Dilution ratios of about 20-30 X were required to bring the concentration within range of the analyzer.

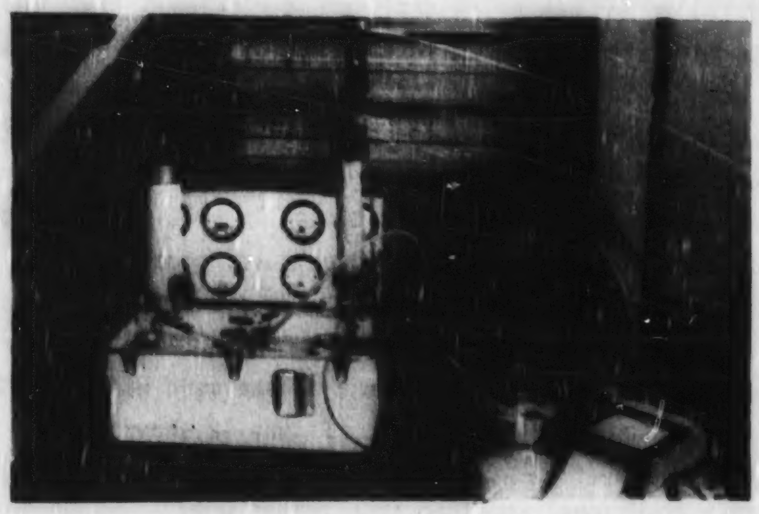

Figure 4. Instrumental setup upstream of the baghouse showing the sample port, diluter, 2 diffusion driers, the EASA, and the strip chart recorder.

Figures 5 and 6 show upstream distributions measured on Nov. 12 and Figure 7 shows the downstream distribution measured on Nov. 12. Note that the concentration scale is 5000X more sensitive in the downstream graph. Except for the peak at 0.015 μm on the downstream distribution which I will discuss later, the shapes of the distributions are similar.

Figure 8 shows the upstream distribution and Figure 9 shows the downstream distribution on Nov. 13. All 5 sets of data were taken with about 11 MW (near full load) generation on Boiler No. 2. Note that Nov. 13 had similar data to Nov. 21 with some rather minor differences, including a much lower peak around 0.015 μm on the downstream data. Note that most of the volume, and therefore mass, of the aerosol is contained by particles greater then 0.1 μm.

Figure 10 shows the volume distributions plotted with a log concentration scale. We see that the six averaged sets of data plotted here, representing 32 separate size distributions, are quit consistent.

Figure 11 shows the penetration of the baghouse for the three pairs of data; two on Nov. 12 and one on Nov. 13. Note that penetration is about 0.0001 between 0.1 and 1.0 μm and may be slightly greater below 0.1 μm. Although the measurements may certainly be a factor of 2 in error, the baghouse definitely has high efficiency, even for particles below 0.1 μm.

An important point regarding the data reduction: all distributions shown are averages of multiple analyzer measurements. Since the concentration of aerosol in the stack varies by typically ± 10 percent from one 10-second period to another, even with good combustion control; any single distribution may not be very accurate. By averaging a number of measurements, preferably five or more, much greater accuracy can be obtained. The use of a strip chart recorder allows one to see more easily if the concentration level is changing during a run. Figure 12 shows typical strip chart traces of upstream effluent with the EASA.

PROBLEMS, POSSIBLE SOLUTIONS, AND SUGGESTED FUTURE WORK

The two downstream distributions shown earlier, especially on the Nov. 12 data, show a very significant peak in the measured volume distribution between 0.01 and 0.018 μm. It should be emphasized that the distributions shown are volume distributions, not surface or number distributions. The

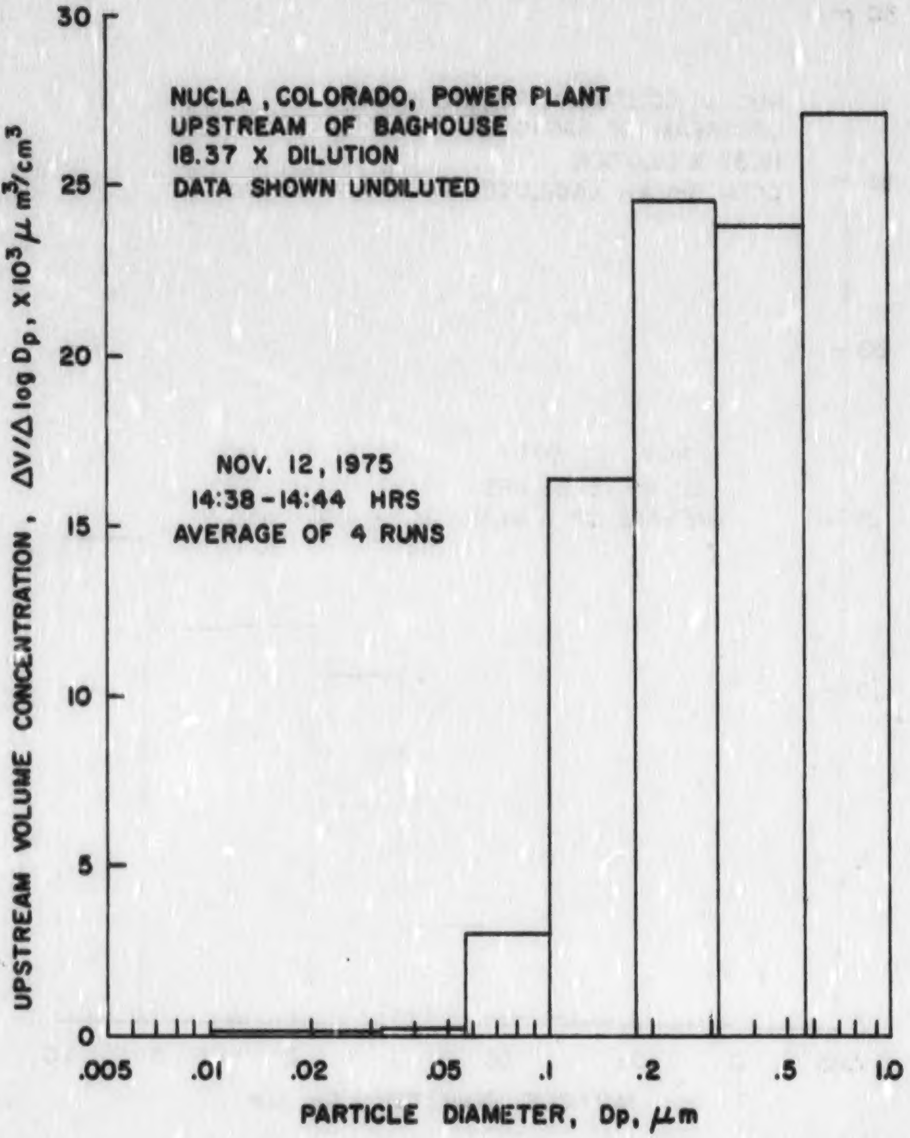

Figure 5. Upstream particle size distribution, 14:38 - 14:44, Nov. 12, 1975.

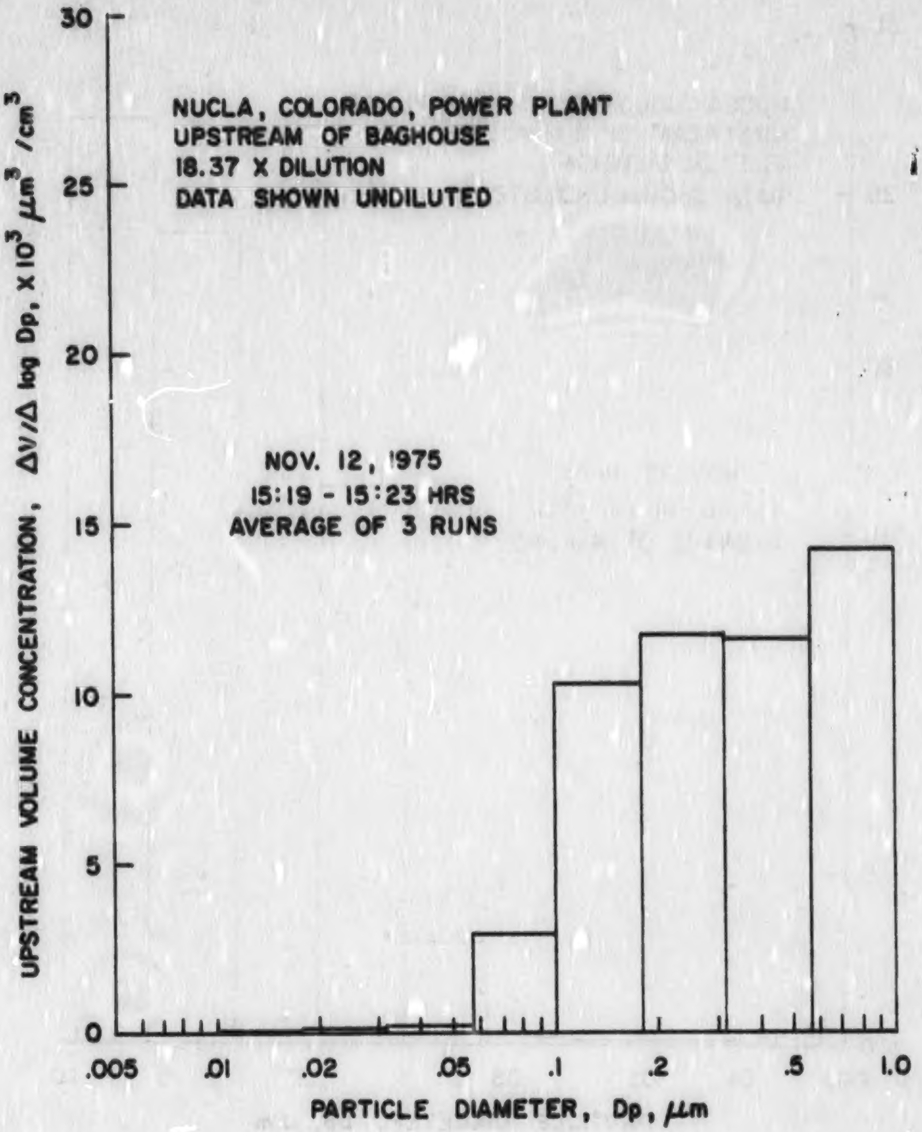

Figure 6. Upstream particle size distribution, 15:19 - 15:23, Nov. 12, 1975.

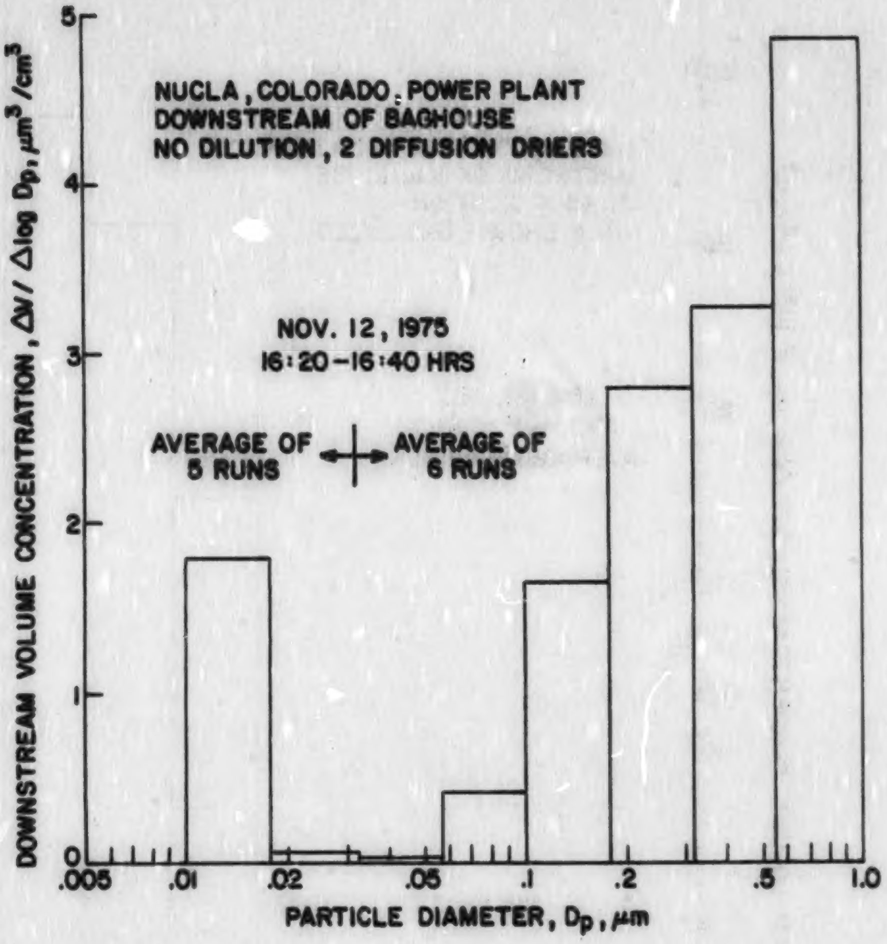

Figure 7. Downstream particle size distribution, 16:20 - 16:40, Nov. 12, 1975.

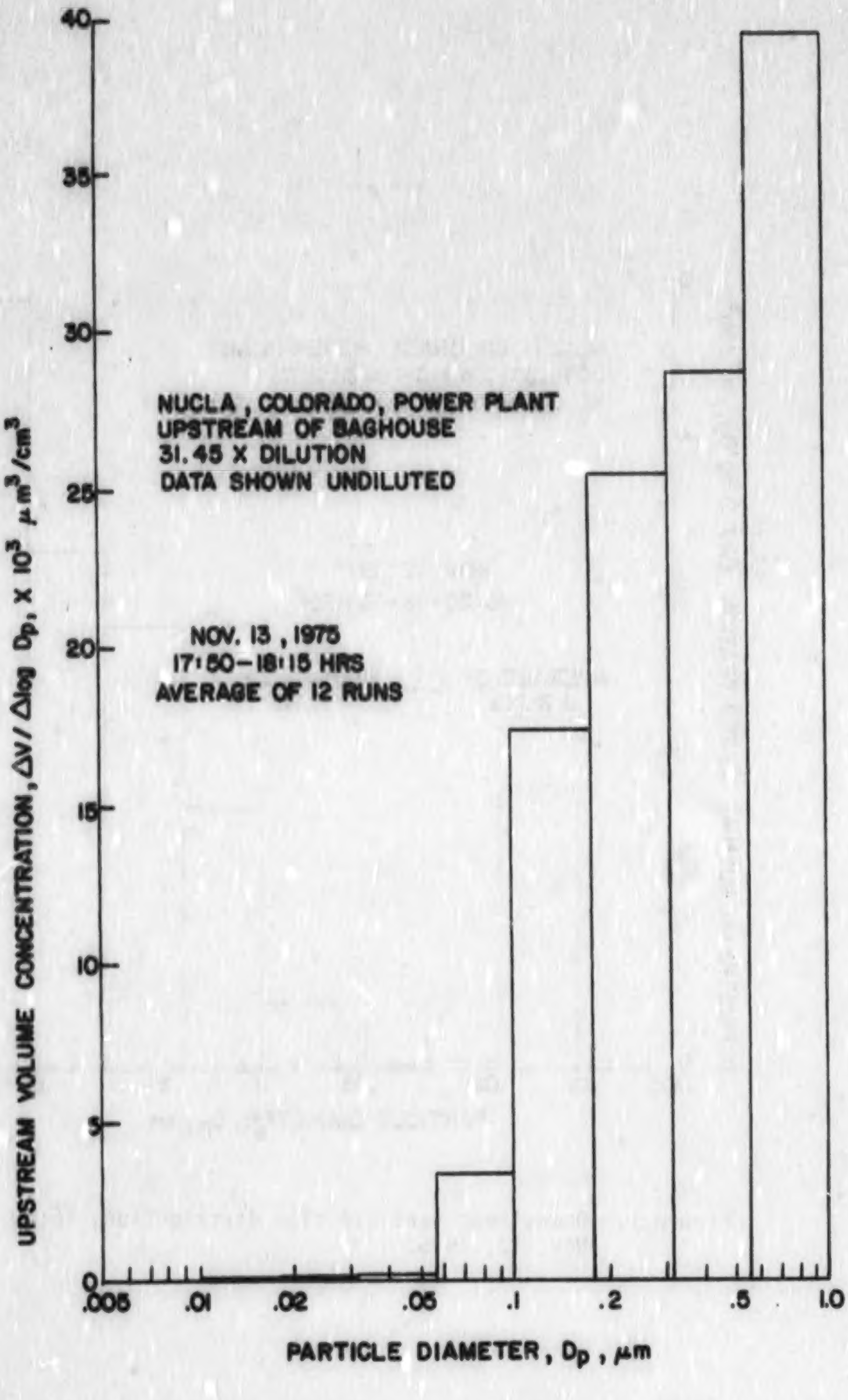

Figure 8. Upstream particle size distribution, 17:50 - 18:15, Nov. 13, 1975.

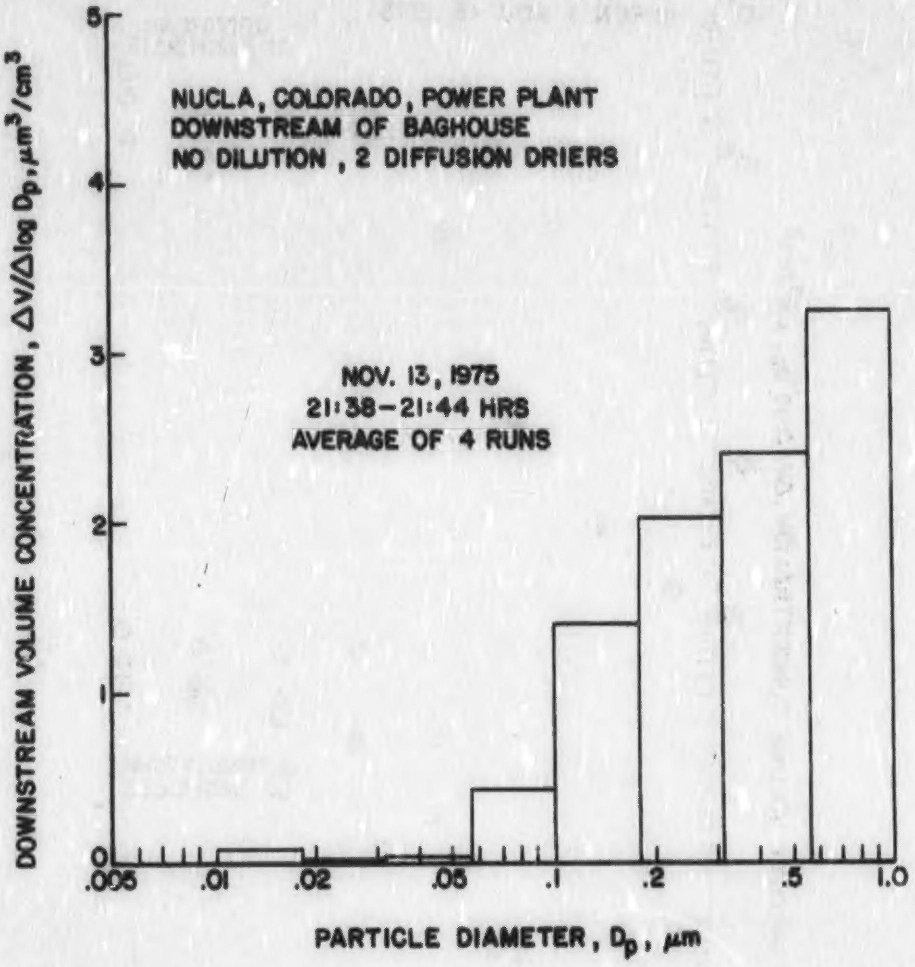

Figure 9. Downstream particle size distribution, 21:38 - 21:44, Nov. 13, 1975.

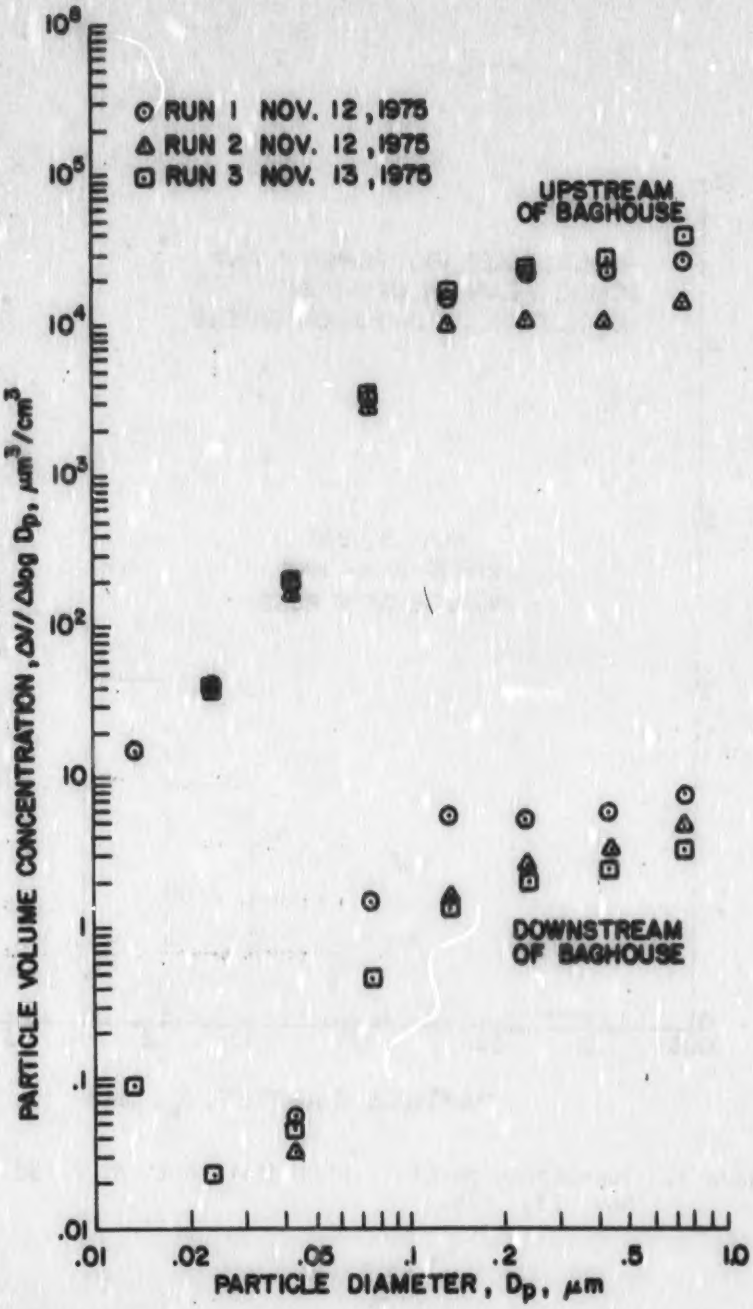

Figure 10. Composite particle size distribution, Nov. 12-13, 1975.

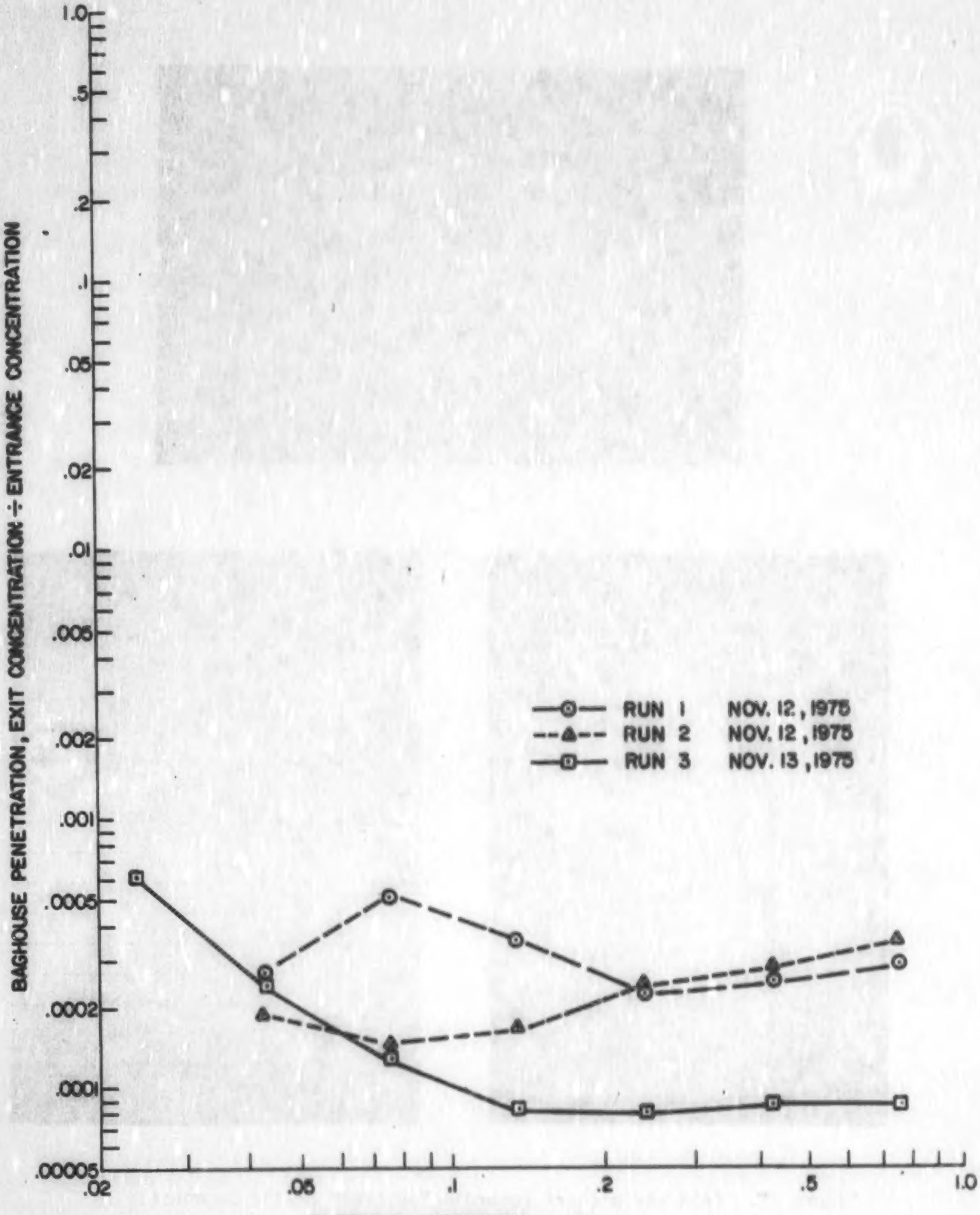

Figure 11. Baghouse penetration of submicron particles.

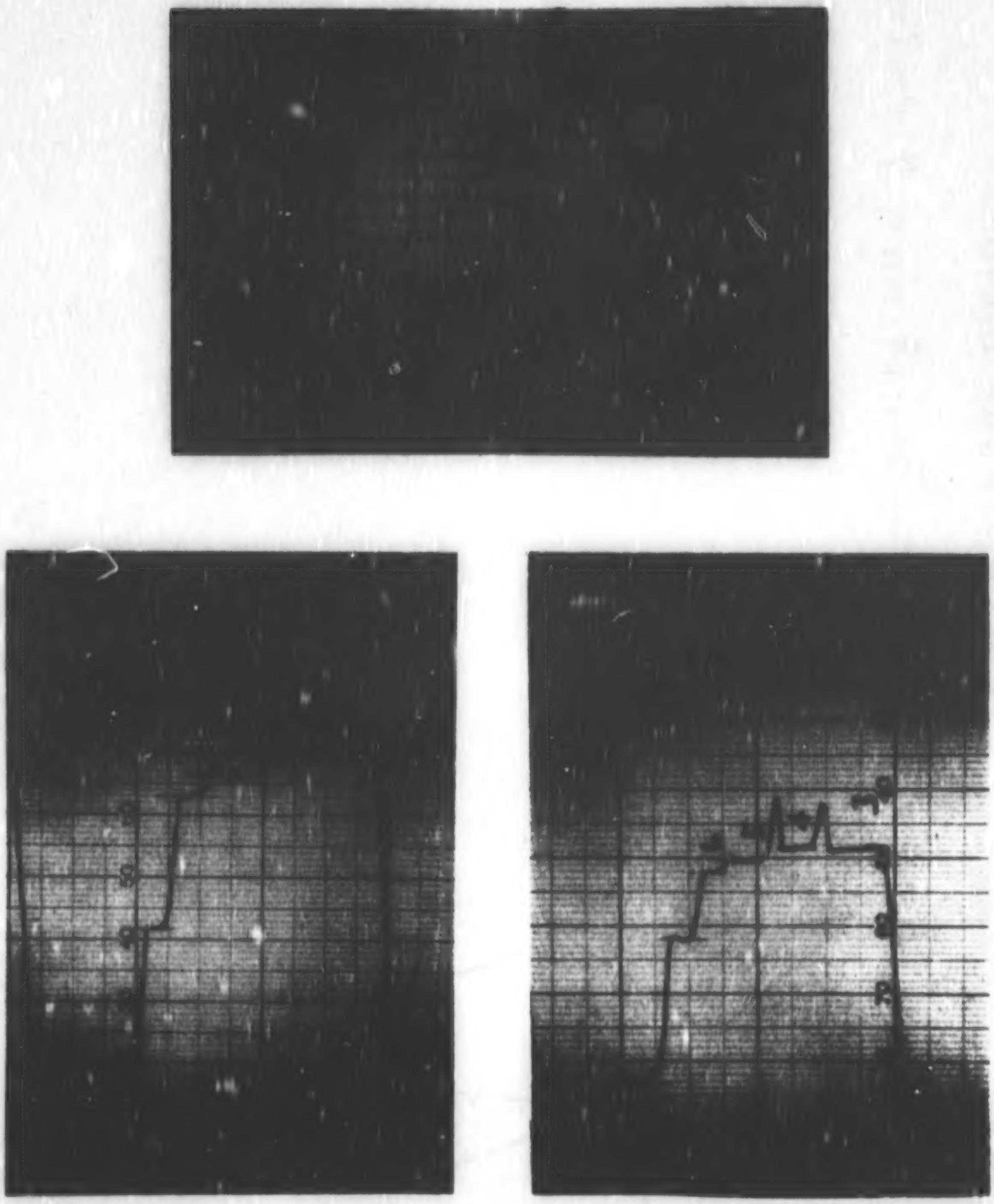

Figure 12. EASA strip chart recording upstream of the baghouse.

overwhelming number of particles in the 0.01 - 0.018 μm size range on the Nov. 12 data accounts for over 99 percent of the total measured number concentration. The only way we could accurately measure this distribution was to record this channel separately and then increase the chart recorder sensitivity to get the size distribution of larger particles. I have rarely seen an aerosol in this size range which is so monodisperse.

Figure 13 shows a typical downstream strip chart recording of the entire size range,and Figure 14 shows the trace for 0.03 μm and larger.

Particles in the 0.01 - 0.018 μm range are probably produced by condensation within the sampling system shortly before the aerosol enters the analyzer. The aerosol almost certainly is not produced in the analyzer itself because the nuclei counter also detected the particles at the inlet to the diffusion battery. Joe McCain of Southern Research Institute has also seen this aerosol and suggests that it is an acid mist, possibly H_2SO_4. He can usually get rid of the very small particles by sufficient dilution. We could not dilute the downstream samples to prevent condensation because the undiluted aerosol concentration was already approaching the lower detection limit of the analyzer.

I feel that the data above 0.05 μm are representative of what is in the stack because the volume distribution reaches a very low level around 0.05 μm and then increases, an indication of 2 independent, nonoverlapping distributions. We collected a yellow-tinted liquid in a water trap between the 2 diffusion driers which smelled like acid, but we did not analyze the liquid. Since this aerosol is casting some doubt on the validity of the data below 0.05 μm, some work should be done to identify it and prevent it.

The possibility that the 0.01 μm droplets are generated in the sampling system is just one illustration of the need for careful development and evaluation of the sampling system for submicron aerosols. Since particles below 0.1 μm are governed by diffusion, coagulation, and condensation rather than gravity and inertia; a sampling system optimized for 1-10 μm particles will not necessarily work well for particles below 0.1 μm.

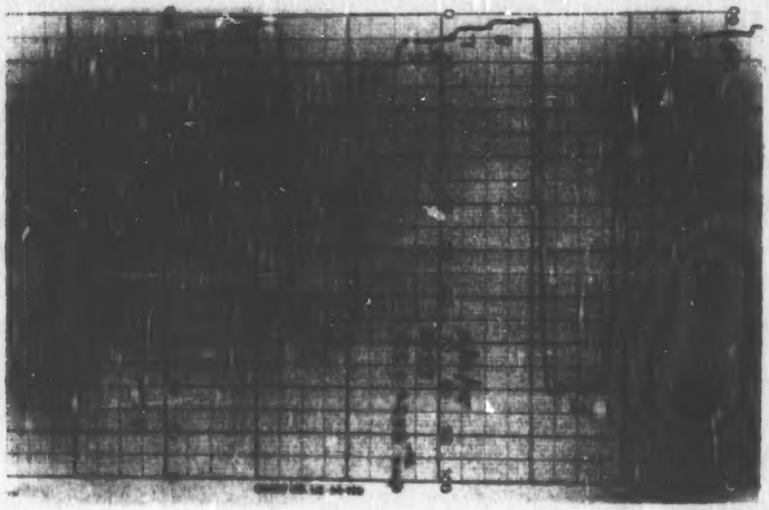

Figure 13. EASA strip chart recording downstream of baghouse showing the data for 0.01 - 0.2 μm particles.

Figure 14. EASA strip chart recording downstream of baghouse showing the data for 0.03 - 1.0 μm particles with the 0.01 - 0.03 μm particles not included.

The data shown earlier were taken during relatively constant plant operation with no shaking of the filter bags. When the boiler was pushed to its maximum of 13 MW, the upstream concentration fluctuated much more and the baghouse went into a continuous shake cycle. The total current measured with the analyzer on the downstream side showed an increase of greater than 2X whenever a bag was shaken. The concentration level did not reach anything resembling constant conditions for at least 10 minutes after shaking. Since a continuous shake cycle calls for one shake every few seconds, no data were taken during shaking.

Similarly, when the coal composition fluctuated, so did the submicron aerosol. When the power plant was operating at non-ideal conditions one day, the submicron aerosol concentration fluctuated by 3-4X at the inlet to the baghouse over a time period of 10 to 20 minutes. No valid size distribution data could be taken during these periods.

Since these unstable periods occur often in a steam boiler, and indeed may be more typical than stable conditions on many boilers, it would seem important to try to characterize the emissions during unstable periods. Batch sampling, as illustrated in Figure 15 with the SRI sampling system, can be used to obtain submicron aerosol samples during unstable effluent conditions. The submicron instruments can then operate on a stable batch of aerosol immediately after the batch sample is taken. This technique has been used successfully for measuring other combustion aerosols such as cigarette smoke, candle flames, propane torches, auto exhaust, and others. I have constructed a batch sampler shown in Figure 15, which can fit into the sampling-dilution system as shown in Figure 16. It uses a flexible plastic bag within a rigid-wall housing. The bag is filled by drawing air from the chamber between the bag and the rigid housing. I have not yet had a chance to try the batch system. One word of caution: do not allow sunlight to reach the aerosol in a batch sampler or you may have a photochemical smog chamber.

It is important to note that some of the cascade impactor data taken in stacks does include bag shaking, electrostatic precipitator rapping, coal composition variations, changes in operating conditions, etc. Thus, data from the submicron near-real-time sensors may not match well with cascade impactor data in these cases.

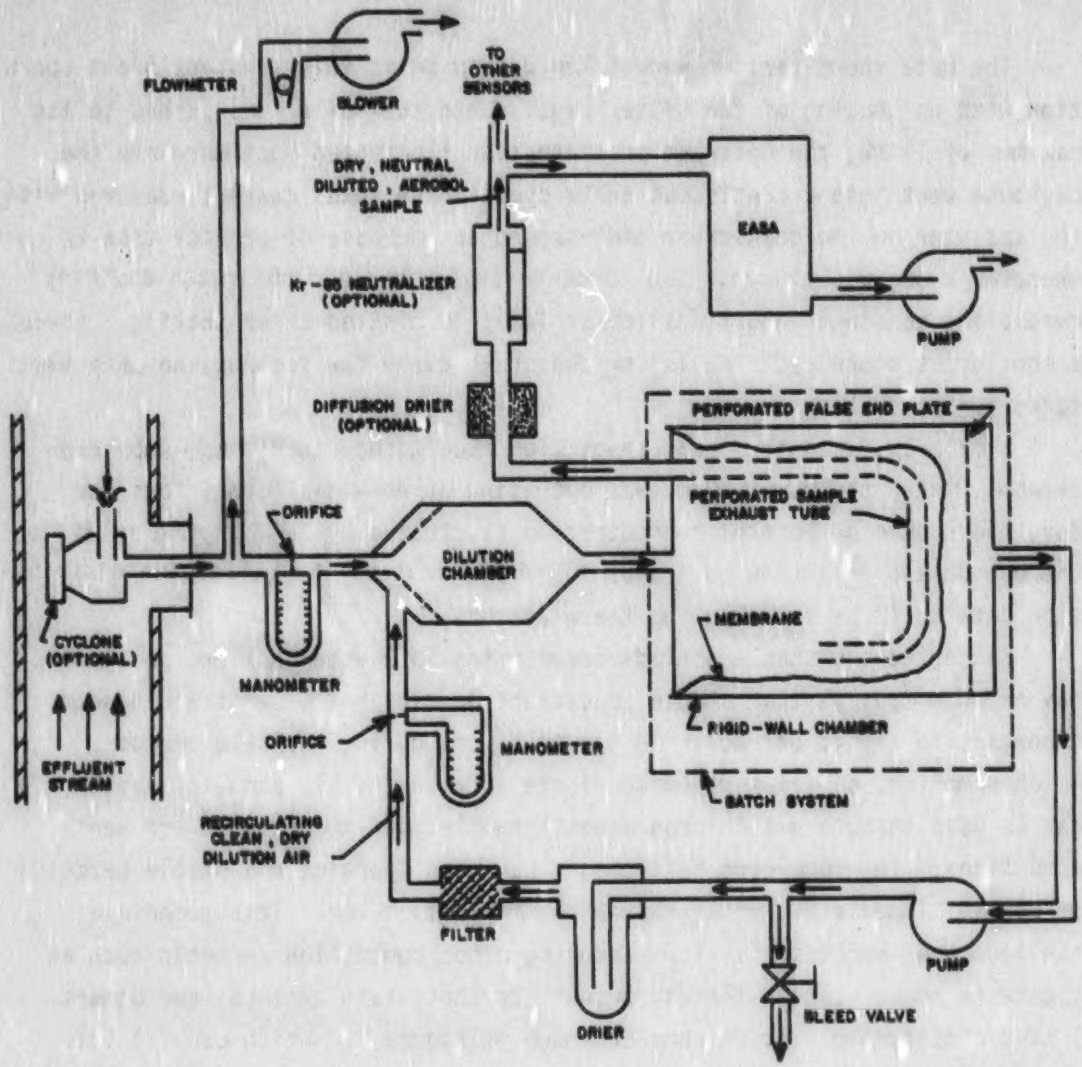
FLOWMETER
BLOWER
TO OTHER SENSORS
DRY, NEUTRAL DILUTED, AEROSOL SAMPLE
EASA
PUMP
Kr-85 NEUTRALIZER (OPTIONAL)
DIFFUSION DRIER (OPTIONAL)
PERFORATED FALSE END PLATE
PERFORATED SAMPLE EXHAUST TUBE
ORIFICE
DILUTION CHAMBER
MEMBRANE
CYCLONE (OPTIONAL)
MANOMETER
ORIFICE
MANOMETER
RIGID-WALL CHAMBER
EFFLUENT STREAM
BATCH SYSTEM
RECIRCULATING CLEAN, DRY DILUTION AIR
FILTER
PUMP
DRIER
BLEED VALVE

Figure 16. Southern Research Institute sampling-dilution system modified with batch sampling system suggested for use on emissions streams with fluctuating aerosol concentration and/or size distribution.

Figure 15. Photograph of a batch sampling system.

The process variations point out the desirability of measuring up- and downstream simultaneously. Since we had only one instrument at Nucla, we settled for the assumption of constant aerosol.

We are confident that we obtained good, valid penetration data from the Nucla Power Plant using the electrical analyzer. However, we feel that this is just a beginning. Much work remains to develop a good sampling, dilution,and cooling system to assure the delivery of a valid sample to the sensor under a wide variety of stack effluent conditions. After that, much work will be needed to evaluate many control devices on many different processes as well as to characterize the submicron aerosol being emitted into the atmosphere.

However, to emphasize the point as strongly as possible, the sample extraction, conditioning, and delivery system is the key to accurate submicron sizing of stack effluents with the electrical analyzer. The majority of measurement errors at this time are attributable to the sampling system, not to the sensor. The future of this technique for submicron particle sizing in stacks appears bright if the sampling problems are solved and fully evaluated.

ACKNOWLEDGMENTS

This work was partially supported by Electric Power Research Institute, Robert Carr, Project Officer. Dr. David Ensor and Mr. Richard Hooper invited me to work with them on their major baghouse evaluation program. I appreciate the opportunity to work with them and to present some of their results in this talk. The field work was performed at the Nucla, Colorado power plant of the Colorado-Ute Electric Association.

DISCUSSION

WAGMAN: I notice that you plot your data out to 1 micron; isn't this beyond the minimum in the mobility-size relationship, so that you have some overlap with the smaller size particles?

SEM: The minimum in the mobility curves occurs at something like 1.5 or 2 microns, and certainly the data are not as accurate up around 1 micron as they are around a tenth of a micron, for example. If I were to apply error bars for the aerosol that was reaching the instrument, I would apply something on the order of ± 50 percent by the number for the largest size that I showed and perhaps more on the order of ± 15 to 20 percent around a tenth of a micron. When you get down to 0.01 micron, I would apply something like a 50 percent error again.

WAGMAN: How do you presently define the size that you get from a mobility analyzer? In a cascade impactor, for example, we refer to it as inertial or aerodynamic diameter. How do you define the size that you get from a mboility analyzer?

SEM: The size is not a nice clean characteristic like aerodynamic diameter, but it is not all that different from it. The instrument is calibrated by using monodispersed aerosols, which are essentially spherical, and feeding them to the instrument in a number of sizes of monodispersed aerosols. So really, it's an equivalent size based on the spherical particle and it's close to a diameter squared kind of relationship. So it's not all that far from an aerodynamic diameter.

WAGMAN: Isn't there a problem here depending on which material you select as your calibrating material? You're tying it into a material of that composition, in as much as it would be a dependence of charging characteristics on the composition of the particles that you happen to use?

SEM: The instrument uses diffusion charging, rather than field charging. In an electrostatic precipitator in a power plant, composition certainly does make a difference because you're using primarily field charging there. The instrument uses diffusion charging, which strictly depends on the statistical chance of an ion approaching a particle with essentially enough momentum to stick to it.

WAGMAN: But the question of whether it sticks to it may very well depend on composition.

SEM: Not for ions on particles in this size range. It's essentially independent of composition.

WAGMAN: I have spoken to Earl Knutson about this matter. He actually conducted work some years ago on this. He said that he did find a dependence.

SEM: I think that you'll find, though, that that dependence was very small. We're talking about a 5 or 10 percent kind of dependence from perhaps a metallic particle which was very conductive to a plastic type particle, for example.

_____: Gil, you do not have an electric field in the charging region?

SEM: There is a very small electric field, about 50 volts across 1 centimeter. That's all.

OLIN: Two-stage charging generates them at high voltage and then moves them across the gap at low voltage.

LILIENFELD: What is the space charge field?

SEM: The NT product is 10^7.

LILIENFELD: Do you calculate the space charge field?

SEM: You can calculate the charge level, and that comes out to something very near NT = 10^7. But we also measure the current which is flowing across the aerosol gap, and so actually we're measuring the ion current which is passing through the very fixed volume that the aerosol is passing through. Those two numbers tend to agree with each other within a few percent. (David Piu of the University of Minnesota has determined that space charge contributes about 15 percent to the field in the charging region).

LILIENFELD: Are the 50 to 100 volts per centimenter the result of the space charge?

SEM: No, that's strictly an applied voltage to get the ions to move across the aerosol stream.

LILIENFELD: Well, I think that the space charge could be several times higher than that; the field produced by the space charge.

SEM: You'll have a minor effect due to that field, certainly. But an NT of 10^7 fully predominates on the charging mechanism. I have several references which I can refer you to on questions of the design and theory and the calibration of the instrument in case you're interested. (See Pui, D.Y.M., Ph.D. Thesis, University of Minnesota, Mechanical Engineering Dept., 1975).

WAGMAN: Have you ever used this instrument downstream of an electrostatic precipitator? Do you have any problems there? I wondered because Bill Kuykendal this morning was talking about our troubles of using it downstream for a particulate which was charged.

SEM: Joe, can I refer that question to you? I haven't.

MCCAIN: We've done a limited amount of work with comparisons of diffusional results with this--apparently successful. We use charge neutralizers in the diluters. We don't have one at point of entry into the probe.

_____: Did you use Krypton 85?

MCCAIN: No, Polonium 210.

KUYKENDAL: I think that may have been part of our trouble. We were in a time tight experimental procedure and we just didn't have time to optimize the sampling system. We tried the one charge neutralizer that we had available, a Krypton 85 source. We were not using dilution. It didn't work.

SEM: I think, also, in a place where you have a powder which has been redispersed, the submicron particles are not redispersed as well as are the particles above 1 micron. You could have a very definite difference due to a small change in relative humidity, and how well you are dispersing the submicron particles which would definitely give you trouble if you are trying to size them.

MCCAIN: Did you (Kuykendal) have a way to knock out relatively large particles ahead of the mobility analyzer?

KUYKENDAL: Other than in the sampling system itself, we didn't.

SEM: It is a good idea, if you have predominately large particles, to knock out the particles with something, let's say a 2 micron cutoff device, either cyclone or a good impactor. Any other questions?

ENSOR: Our data are really in preliminary form; there are many more things we can do. We have some preliminary chemical anaysis here from impactors, which indicates it was sulfur-rich brine on the plates. There is a possibility that we may have been below the acid dew point in the stack itself, and it may not have been particles in the stream. Again, these are all preliminary data.

MCCAIN: Well, we've seen it where we've definitely introduced it in the train. We made a change of dilution of as little as 10 or 15 percent, and this resulted in a change in concentration with the condensation nuclei counter by a factor of almost 10^6.

ENSOR: My question is, how do you really know what was created in the train? I mean you're manipulating something in the train and suddenly things change. That's not conclusive proof. What I'm saying is, this is a potentially serious problem in submicron testing, and it's not always straightforward.

PANEL DISCUSSION ON DIFFUSION BATTERIES

Joseph D. McCain, Southern Research Institute

SPARKS: We'll start the discussion of diffusion batteries and their use in-stack, their use out in the field to take size distributions somewhere around .01, .05 to a few tenths of a micron particle diameter. They fill in the gap below the impactor region. The way that we'll do this is to allow Joe [McCain] of Southern Reserach, Dave [Ensor] of Meteorology Research, Seymour [Calvert] of APT -- Pedro [Lilienfeld] or Reed [Cass] does somebody from GCA want to talk about theirs?.

LILIENFELD: Perhaps.

SPARKS: Perhaps. Well, we'll let these three, anyway, talk about the things that they are doing. Joe [McCain] and Dave [Ensor] both have more of a classical diffusion battery configuration, parallel plates. Seymour [Calvert] has decided that chemical engineers knew something about mass transfer to cylinders, so he developed a new geometry of diffusion batteries. If each one of you could maybe take about 10 minutes to 15 minutes at the most to say what you're doing, then we'll let Pedro [Lilienfeld] decide if he wants to say anything, and let anybody else make a comment. Joe, if you want to go first.

MCCAIN: We started in this several years ago, trying to characterize some fine particle emissions from a pulp mill, and some of the equipment we made then is still in use today. It was pretty unconventional then for stack sampling. We were probably conservative in our diffusion battery design, and you'll see the result of that shortly. Let me run through a little bit of the history of our field work with some slides. For those of you who aren't familiar with a diffusion battery system, the detectors that are used for diffusional sizing are not capable of coping with the particle concentrations that are normally encountered in flue gases and extensive sample dilution is necessary as Gil Sem has already mentioned. Figure 1 is an earlier version of the slide that Sem used showing the essentials of

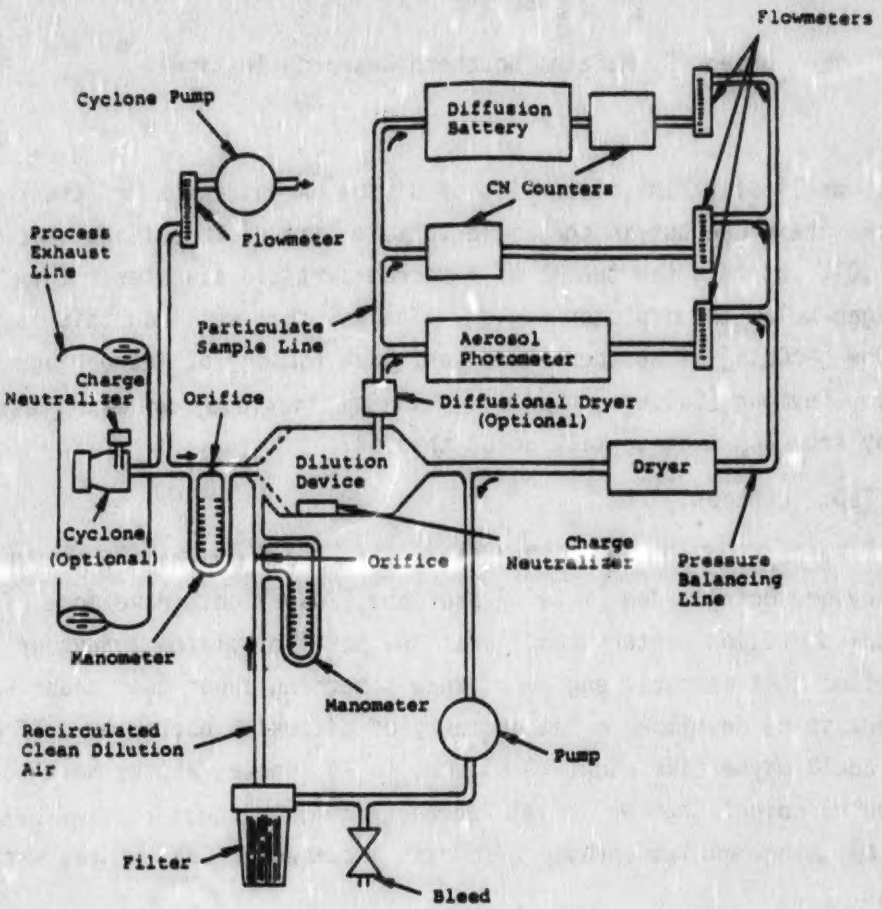
Flowmeters
Cyclone Pump
Diffusion Battery
CN Counters
Flowmeter
Process Exhaust Line
Particulate Sample Line
Aerosol Photometer
Charge Neutralizer
Orifice
Diffusional Dryer (Optional)
Dilution Device
Dryer
Cyclone (Optional)
Charge Neutralizer
Orifice
Pressure Balancing Line
Manometer
Manometer
Recirculated Clean Dilution Air
Pump
Filter
Bleed

Figure 1. Optical and diffusional sizing system.

Reproduced from best available copy.

our sample conditioning system which involves a metered flow into a dilution device from which a secondary sample is taken to the various particle monitors (an optical particle counter, CN counters reading total concentration of ultrafine particles and diffusion batteries with a condensation nuclei counter downstream of them.)

The diffusion battery operates on the principal of removal of particles by Brownian motion, which results in the migration of particles to the walls of the battery. The diffusion batteries which we have generally used in our field work are of the parallel plate type, using one millimeter separation between the plates. The residence time for gases flowing through those batteries ranges from a few seconds upwards to an hour. They remove particles on the order of .01 to 0.3 μm in size. The diffusional rates of moderately large particles are quite slow, and consequently the required residence time in the battery is long, which results in long delay times in the measurements. This does not make the method particularly suitable for obtaining data on a rapidly fluctuating source. Seymour [Calvert] will talk, I'm sure, on the screen type which has much shorter transport times. We are trying some screen type diffusion batteries too, but he's got a lot more experience with them than we do.

We try to keep all aerosol transport lines as short as possible to minimize losses in the sample lines prior to entry into the diluter or instrumentation. A charge neutralizer is used in the diluter to remove electrostatic charges from the particles at that point, but it would be better to have it in the stack. We have used Climet and Royco optical particle counters with pulse height analyzers, General Electric condensation nuclei counters (which weigh about 150 lbs) and an Environment-One condensation nuclei counter that we've had moderate success with (you can't use it as it arrives from the factory). It uses pneumatic valves that operate on pressure differentials of about 2 inches of water, and if you put the inlet or exit of the counter at a differential pressure to ambient on the order of 2 inches of water or greater, it'll lock the valves open or shut and it won't function at all. We've modified ours to at least partially alleviate this problem. It has been used, when well treated, with reasonable success in the field. As a standby device we generally take along a Gardner manually operated CN counter in case all of the automatic ones fail. The G.E. CNC device is a nice rugged instrument, but the 150 lb weight does make it a little big to carry around (G.E. is now making a smaller, lighter version).

Our diffusion batteries consist of a small one with 12 1-mm wide, 10-cm high, 38 cm long channels and four large ones, each of which has 96 channels of the same dimensions as those on the small one. Each of the large ones weighs about 65 lb . Figure 2 illustrates one of the diffusion batteries and shows its penetration characteristics.

We could have made them a good deal more compact and somewhat lighter but we were very conservative. We wanted them to work the first time out and we wanted to be sure that we didn't have any manufacturing problems in the critical dimensions. We used a slot geometry with the long axis vertical so that settling wouldn't play a significant role in particle losses. As sample extraction is necessary with these instruments one of the problems one immediately encounters is probe losses. Figure 3 shows a plot of theoretical probe losses based on settling losses in long tubes at one extreme in size and diffusional losses at the other. For moderately high flow rates, on the order of 0.5 or so (10 l/min) even with a 10-ft probe, the losses are generally inconsequential over the range 0.01 to 1 micron. Thus, sample extraction for this size range does not introduce significant errors. Electrostatic effects might play an additional role. We haven't really explored that, other than in some limited experiments in the lab. In those experiements they did not seem to be particularly bad over the range from about a half micron down to 0.01 μm in probes of the dimensions that we use and at the flow rates that we use. Electrostatic effects in diffusion batteries themselves can be very significant and charge neutralization prior to passing the aerosol through the diffusion battery is required. One can get electrostatically induced losses of 90 percent in a diffusion battery very easily for some sizes.

Figure 4 shows the overlap in size distribution on a number basis obtained using three different methods. The plot shows the differential size distribution (dN/d log D) versus size as obtained with cascade

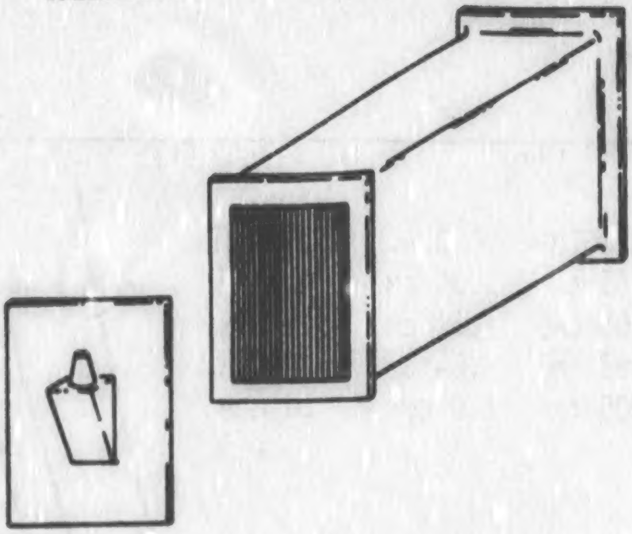

Figure 2a. Parallel plate diffusion battery. The batteries have 12 or 96 channels 0.1 x 10 x 48 cm.

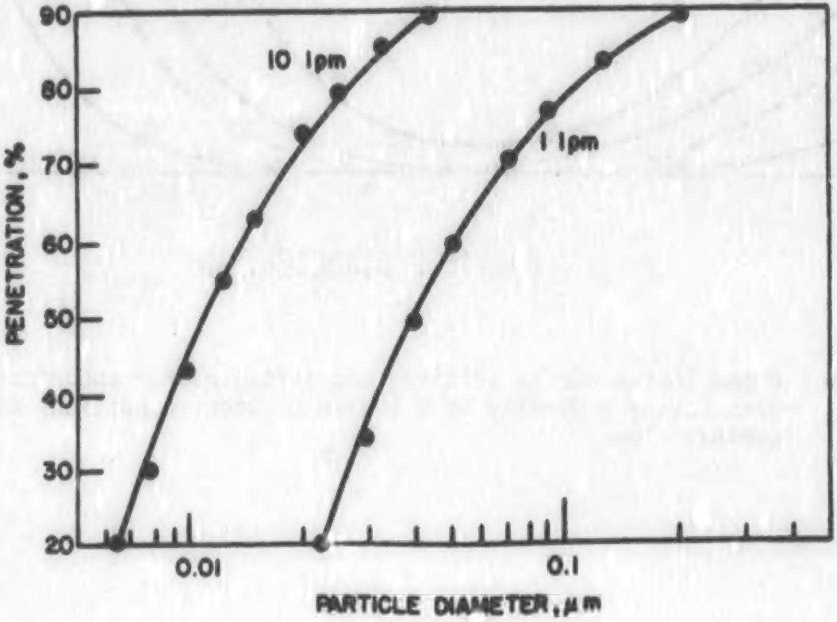

Figure 2b. Penetration curves for monodisperse aerosols (12 channels, 0.1 x 10 x 48 cm).

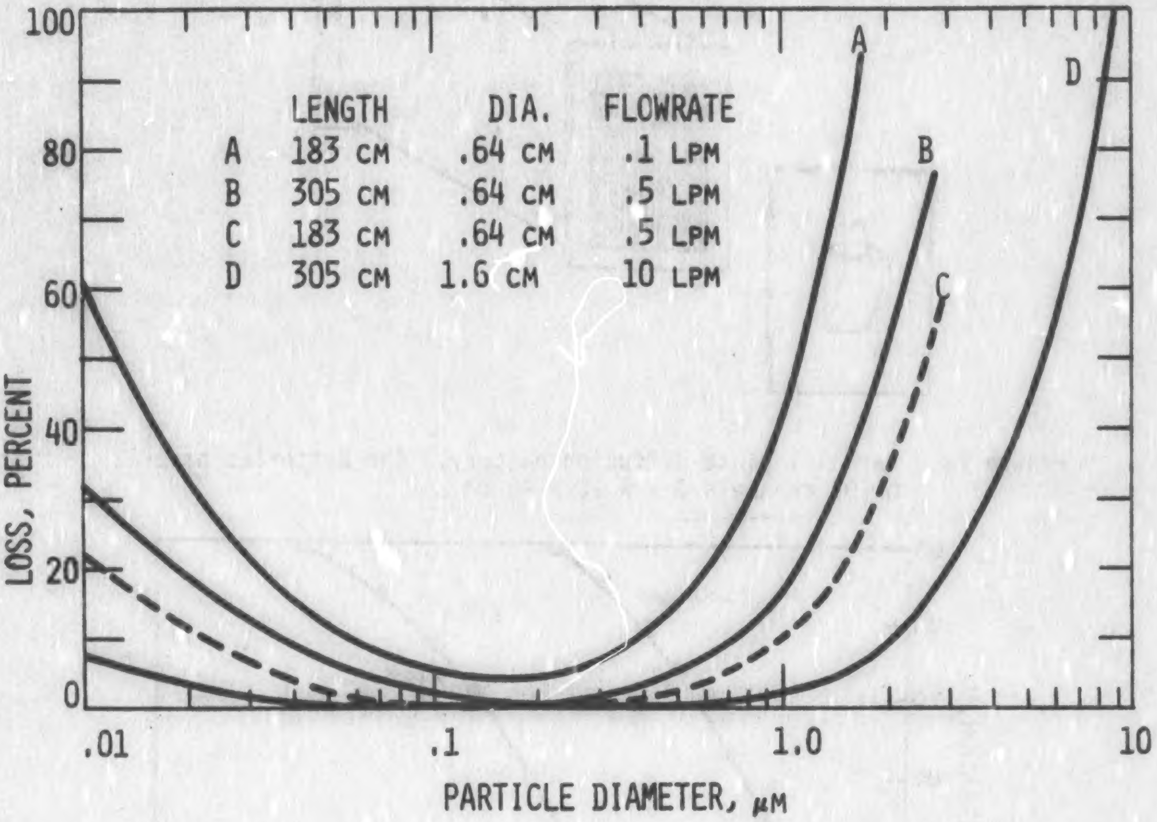

Figure 3. Probe losses due to settling and diffusion for spherical particles having a density of 2.5 gram/cc under conditions of Laminar Flow.

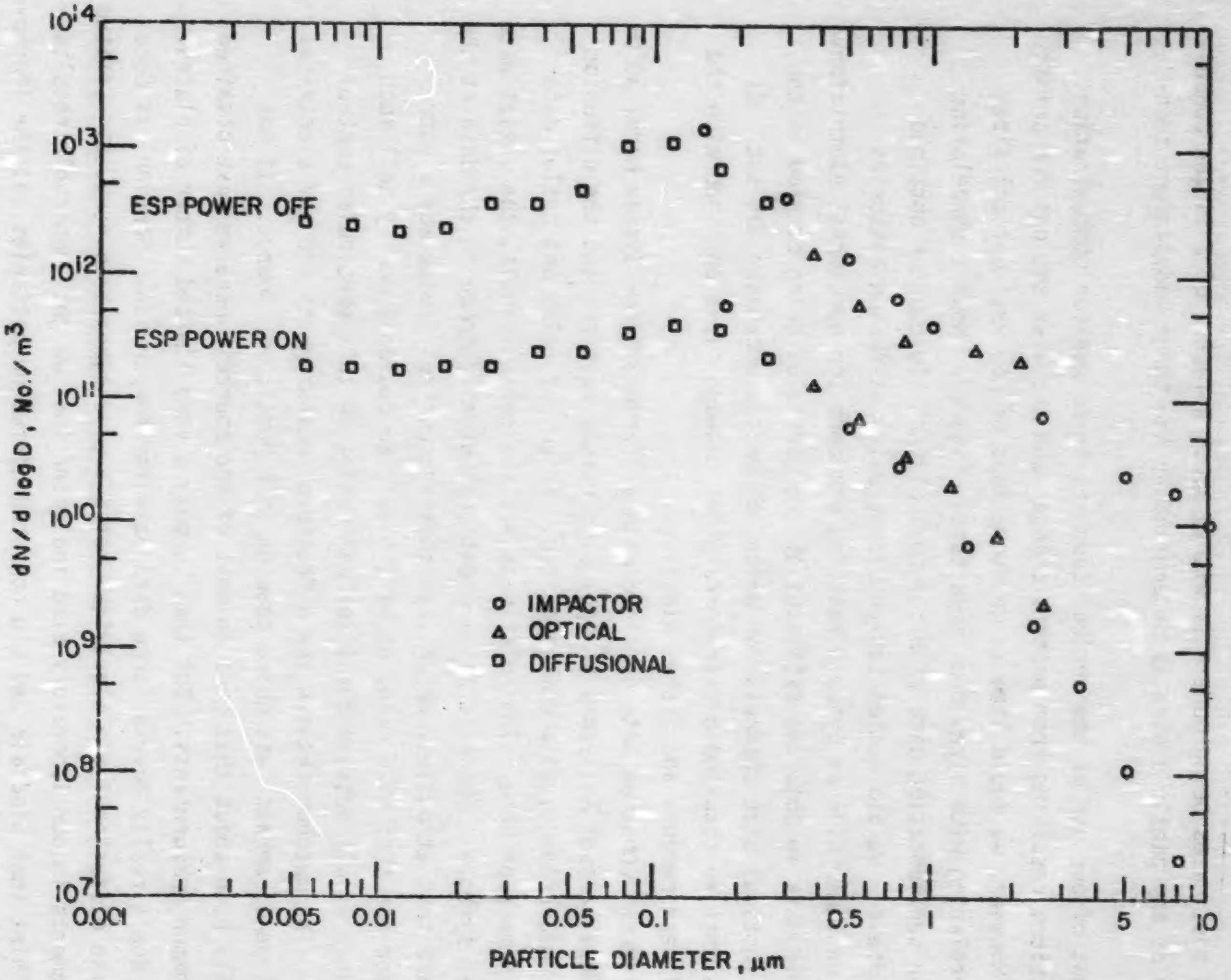

Figure 4. dN/d log D vs. D at inlet and outlet of electrostatic precipitator installed on a coal-fired power boiler.

impactors starting at about 10 μm and going down to about 0.5 μm, optical particle counters in the .3 to 1 μm range and diffusional sizing on down to .01 μm. The overlap and matchup of the size distributions is pretty good in most cases being generally within about a factor of 2 in the concentration at any particle size as determined by the three independent techniques.

Most of our use of the optical counters is to monitor concentration fluctuations resulting from process changes and the like and not for primary data. However, we would like to get some idea of how well optical sizes are correlating with sizes from impactors. Figure 5 shows a correlation based on sedimentation data with the optical particle counter obtained in the field. We did sedimentation sizing using diffusion batteries tilted on their side as dynamic settling chambers (or horizontal elutriators). From this data we obtained estimates of the particles being counted in the various optical size channels for which the particle counter was set. It appears that we obtained a fair correlation between sizes obtained with the optical measurements and Stokes diameters.

Figure 6 compares data obtained with a Thermo-Systems System Model 3030 Electrical Aerosol Analyzer, an optical particle counter, and the diffusion battery measurements at a common source. We've got inlet and outlet data on the same plot here. The inlet data are the solid symbols, the outlet data the open symbols. There is not a great deal of difference in the data at the inlet and that at outlet, which says that the control device was a poor collector for that size range of particles. We couldn't really tell much difference at all between the inlet and outlet on this particular control device. The matchup between the diffusional measurements and the electrical aerosol measurements was quite good on this particular source. It has typically been about that good on most of the sources where we have obtained such common measurements. But that is only a very limited number of places, and we don't really have a large data base for comparison. However, it does look like diffusional and electrical methods give comparable results. Limited experiements in our laboratory also indicated that we got comparable results, and I think that Sinclair and Liu compared diffusion batteries and the Thermo-Systems device and got very excellent agreement during some work at the University of Minnesota.

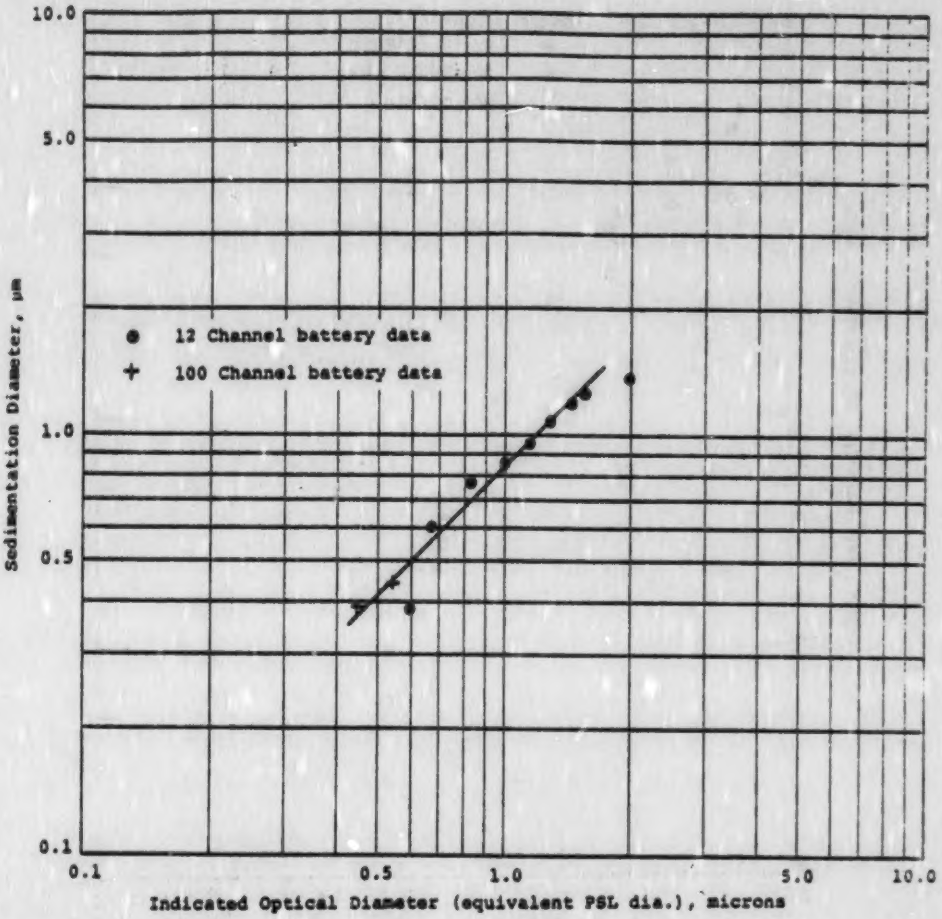

Figure 5. Correlation of optical and sedimentation diameters. Data acquired using fly ash obtained from a coal-fired boiler.

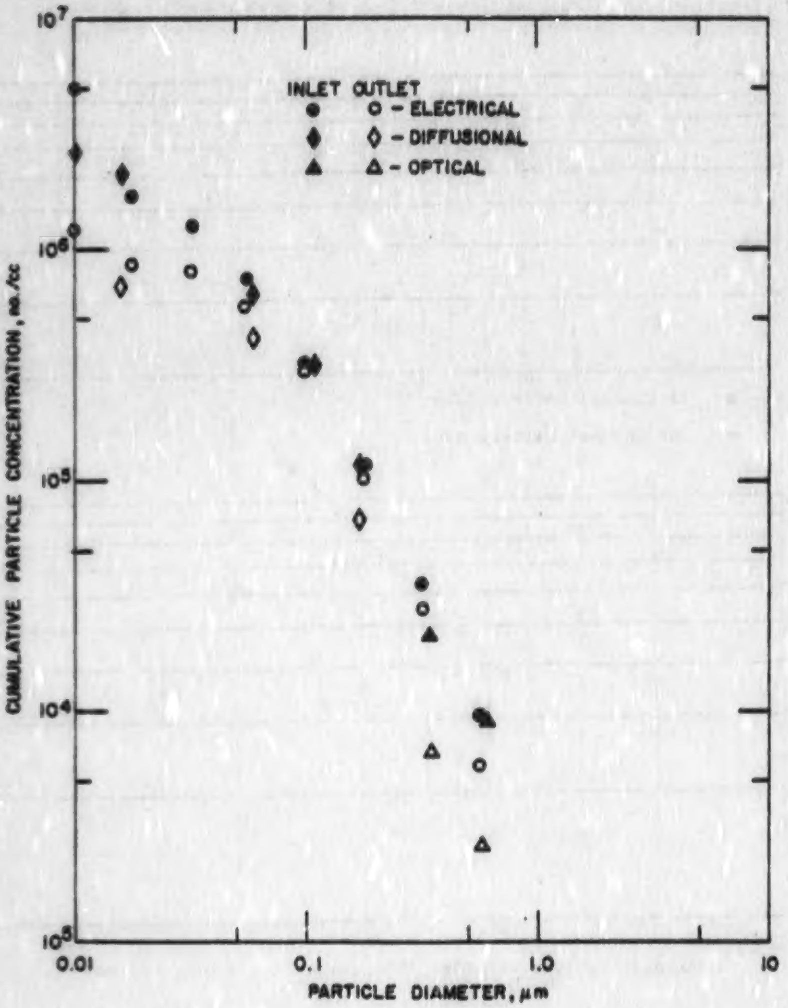

Figure 6. Inlet and outlet size distributions as obtained with optical, diffusional, and electrical techniques.

SEM: I think that what they were doing was using the differential mobility analyzer, which uses a similar kind of principle and comparing that to diffusion batteries. In fact, they are doing more work on it.

MCCAIN: Figure 7 shows the fractional efficiency curve obtained for a precipitator. It shows a very characteristic phenomenon with ESP's dip in collection efficiency in the vicinity of three tenths or a half micron.

We've frequently encountered a problem that Gil mentioned--the apparent formation of an aerosol in our sampling system as the result of what appears to be a vapor phase component of the gas stream passing through the dew point in the sample conditioner and forming a very small characteristic size aerosol. We attribute this to sulfuric acid condensation. It hasn't been demonstrated to really be that. The fact that it appears to be something that is formed in our sampling system is based on the observation 10 percent change in the dilution of the stack aerosol sample has frequently lead to a concentration increase as detected by the instruments of a factor of 1,000 to 10,000 rather than a 10 percent change in concentration. Diffusion battery measurements on such aerosols confirmed what Gil has shown that it was quite small. Diffusion batteries designed to eliminate particles smaller than about .02 microns would remove essentially all of it. Apparently, something had formed in the sample conditioner not in the flue. If it existed in the flue then the small change in dilution would have resulted in small change in indicated concentration by the instruments. It is the very sudden concentration increase with slight change in dilution that leads us to believe that it is a dew point phenomenon (condensation).

____: What did your temperature tell you about it?

MCCAIN: We don't know what the temperatures are precisely. We're taking a 300-320° flue gas and suddenly mixing that with ambient air, and we don't know what the detailed temperature structure is in that mixing process. We are certainly bringing the sample down very rapidly in temperature, and it's a question of whether it mixes fast enough to keep the vapor from ever going over the dew point concentration. We just don't know what the history is through the mixing zone. It is certainly possible for the sample to go

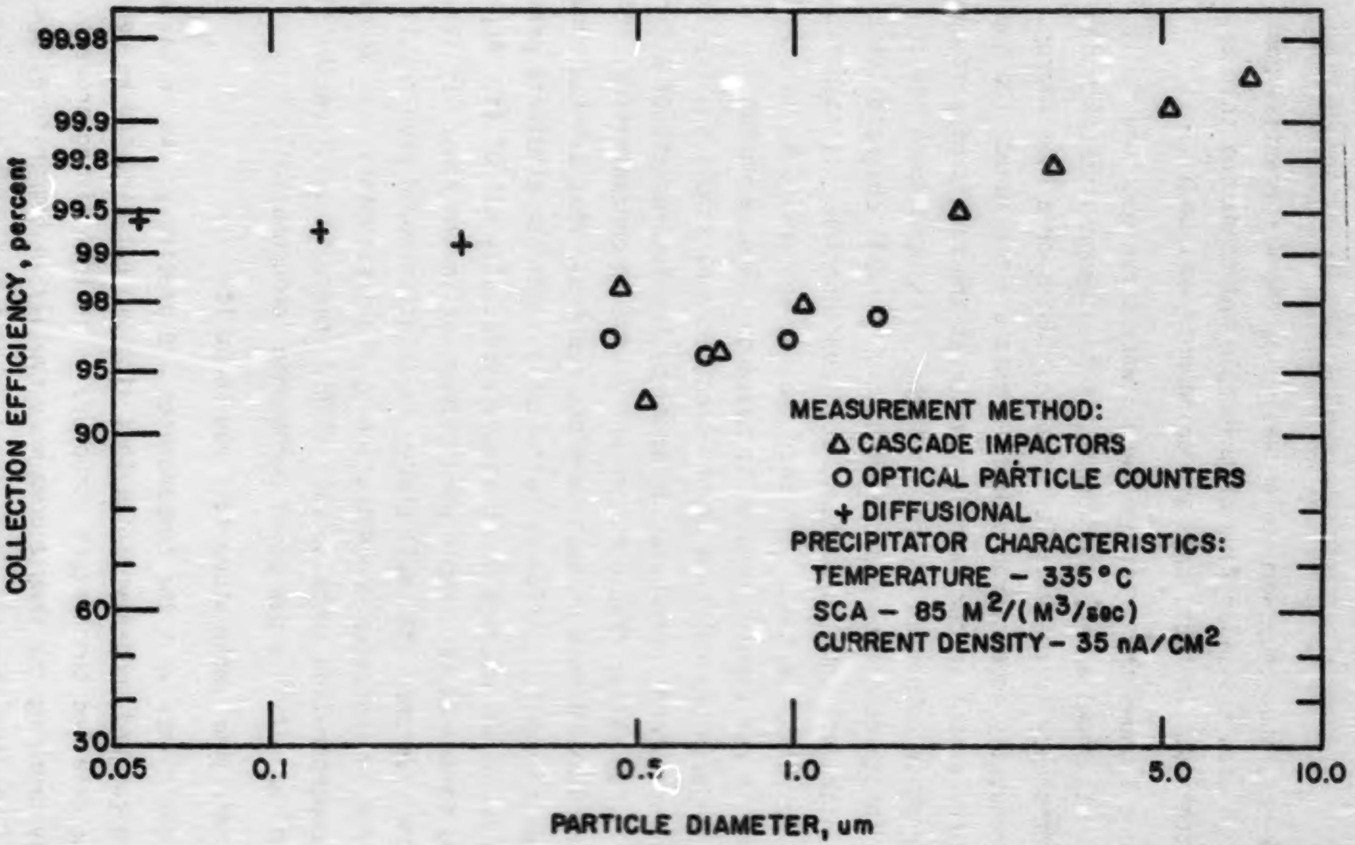

Figure 7. Measured fractional efficiencies for the San Juan hot side electrostatic precipitator with the operating parameters as indicated, installed on a pulverized coal boiler.

through the dew point. It does not always occur, we've had a lot more trouble with it occurring on boilers that are burning a high sulfur coal than on boilers that are burning a low sulfur coal or sources that do not have sulfur present, which is why we speculate that it may be sulfuric acid.

PANEL DISCUSSION ON DIFFUSION BATTERIES

THE METEOROLOGY RESEARCH, INC., EXTRACTIVE SAMPLING SYSTEM FOR SUBMICRON PARTICLES

Dr. David S. Ensor, Meteorology Research, Inc.*

SPARKS: Thank you. Dave, do you want to go next?

ENSOR: The MRI extractive sampling system for control device evaluation is briefly described. The diluter is a three-stage unit with adjustable dilutions up to 2,000:1. The diffusion battery is 5 units with particle size cuts from 0.01 to 0.2 microns by diameter. A modified cascade impactor is used as an in-stack precutter.

INTRODUCTION

The objective of this presentation is to discuss the major points of the MRI extractive sampling system. The equipment has been constructed over the past year, tested in the laboratory, and used in a major field test.

EXTRACTIVE SAMPLING EQUIPMENT

In order to determine the penetration of submicron particles through a control device, their effective mass is measured at both inlet and outlet sample locations. Mass measurements are made only after the sample is extracted through a large particle knockout device and diluted for compatibility with particle detection instrumentation. Figure 1 is an illustration of the normal sampling train.

Usually, the sample is extracted at nonisokinetic flow rates due to system design and instrumentation specifications. This should not effect sample collection of the submicron particles of interest.

Sample Train

Precutter

For source sampling, a precutter is used to prevent large particle contamination of the fine particle sample train. For many purposes, a modified

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* Coauthored by Richard G. Hooper, Meteorology Research, Inc.

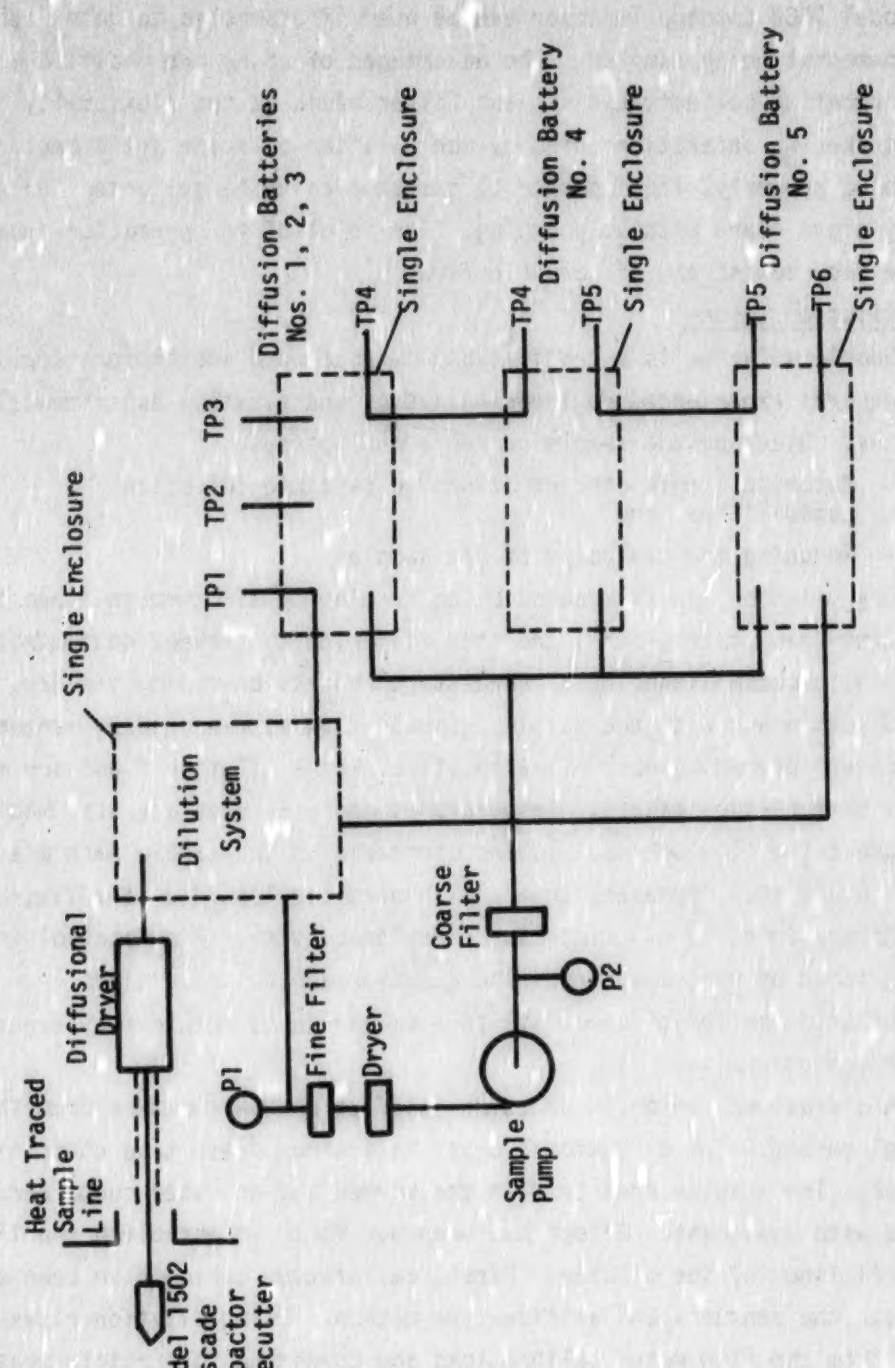
Model 1502
Cascade
Impactor
Precutter
Heat Traced
Sample
Line
Diffusional
Dryer
P1
Fine Filter
Dryer
Sample
Pump
P2
Coarse
Filter
Dilution
System
Single Enclosure
TP1
TP2
TP3
TP4
Single Enclosure
Diffusion Batteries
Nos. 1, 2, 3
TP4
TP5
Single Enclosure
Diffusion Battery
No. 4
TP5
TP6
Single Enclosure
Diffusion Battery
No. 5

Figure 1. Flow diagram for submicron particle source testing.

MRI Model 1502 Cascade Impactor can be used if assembled to mate with the environmental being sampled. The advantages of using the modified impactor by eliminating collection discs and filter media is the flexibility of determining the D_{50} separation point by manipulation of stage jet diameters. When assembled properly, the impactor allows samples to be collected for approximately eight hours without plugging. The inlet of the precutter-impactor should face downstream of particle flow.

Dilution System

Sample dilution is a critical aspect to a good and proper sampling train and requires experienced engineering design and flexible experimental capabilities. Diluting the sample serves a dual purpose:

- Matching sample concentrations to particle detection capabilities, and
- Reducing the dew point of the sample.

Dry dilution air is created by recirculating air through a bed of $CaSO_4$ desiccant (dew point, -90°F) and then filtering to prevent contamination. Dilution is accomplished by a three-stage process of mixing the dry, particle free dilution air with the sample. Sample flow is measured by venturi-type flow meters preceding each dilution stage, while dilution flows are measured across orifice-type meters. Temperatures and pressures are also monitored throughout the flow scheme. Tubing diameters in the sample path are reasonably large (0.375 inch diameter) to minimize particle loss due to diffusion and tubing lengths are short to minimize sample residence time. Flow control is accomplished by manipulation of the dilution air control valves.

Dilution ratios of about 3:1 to 2,000:1 can be obtained by adjustment of the control valves.

A diffusional dryer is sometimes used to remove mositure from the aerosol stream. The diffusional dryer is a wire screen tube containing the aerosol. The annular area between the screen and an outer container is packed with desiccant. Effort has been put forth to establish the linearity and efficiency of the diluter. First, calibration curves have been established for both the venturi- and orifice-type meters. Using dilution rates determined from the flow meter calibrations and comparing to particle measurements at the inlet and outlet of the diluter gives an estimation of particle loss and/or growth in the diluter.

Diffusion Battery

Diffusion is a process by which particles are removed from a gas; this removal is characterized by the theoretical relationships between particle diffusivity (D_p) and parameters characteristic of the gas. Einstein introduced a theory which applies to particles with characteristic dimensions the same as or greater than the mean free path of the gas molecules, and deduced that particle diffusivity may be expressed as follows:

$$D_p = CKT/3\pi\mu d \qquad (1)$$

Where
D_p = Particle diffusivity, cm^2/sec
C = Cunningham correction factor
K = Boltzman's constant, ergs/°K
T = Absolute temperature, °K
d = Particle diameter, cm
μ = Fluid viscosity, dyne-sec/cm^2.

The diffusion battery is a chamber with a large surface area where the aerosol particles are pushed to the wall with Brownian Diffusion. The basic batter can be either a bundle of small diameter tubes or an array of narrow rectangular ducts.

In a plane-parallel channel (with a cross-sectional length much greater than its width), the concentration ratio or battery penetration for both steady state and unsteady state conditions is expressec by De Marcus' formula:

$$n/n_o = 0.9149 \exp^{-1.885\mu} + 0.0592 \exp^{-22.33\mu} + 0.026\exp^{-151\mu}$$

Where
n = outlet concentration, No-/cm^3
n_o = inlet concentration, No-/cm^3
μ = XD
D = particle diffusivity, cm^2/sec
X = diffusion battery parameter, sec/cm^2.

The battery parameter is:

$$X = \frac{L4hWN}{h^2Q} = \frac{4LWN}{hQ} \qquad (2)$$

Where Q = the volumetric flow rate, cm^3/sec
L = length of battery channel, cm
W = width of the channel, cm
N = number of channels
h = height of the channel, cm.

The battery parameter X includes the physical dimensions of the battery and the gas flow rate which are manipulated to achieve particle separation.

An example of the parallel plate diffusion battery is illustrated in Figure 2. The spacing and number of channels are varied to create the distinguishing parameter characteristic of each battery. Table 1 lists the representative parameters used in a recent study conducted by MRI.

Table 1

Battery No.	Battery Parameter
1	284 sec/cm
2	1065
3	3975
4	23272
5	65274

To obtain fractional penetration information, total particle counts are taken before and after each battery with a condensation nuclei counter (Gardner Assn., Inc.). Also, to add credibility to the data recovered by the CN counter, filter samples are taken before and after each battery; field weighings of these filters provide an estimation of particle density through the battery system. Figure 3 illustrates the flow scheme and test points for any given battery.

Two instruments are used for particle detection in conjunction with the diffusion system.

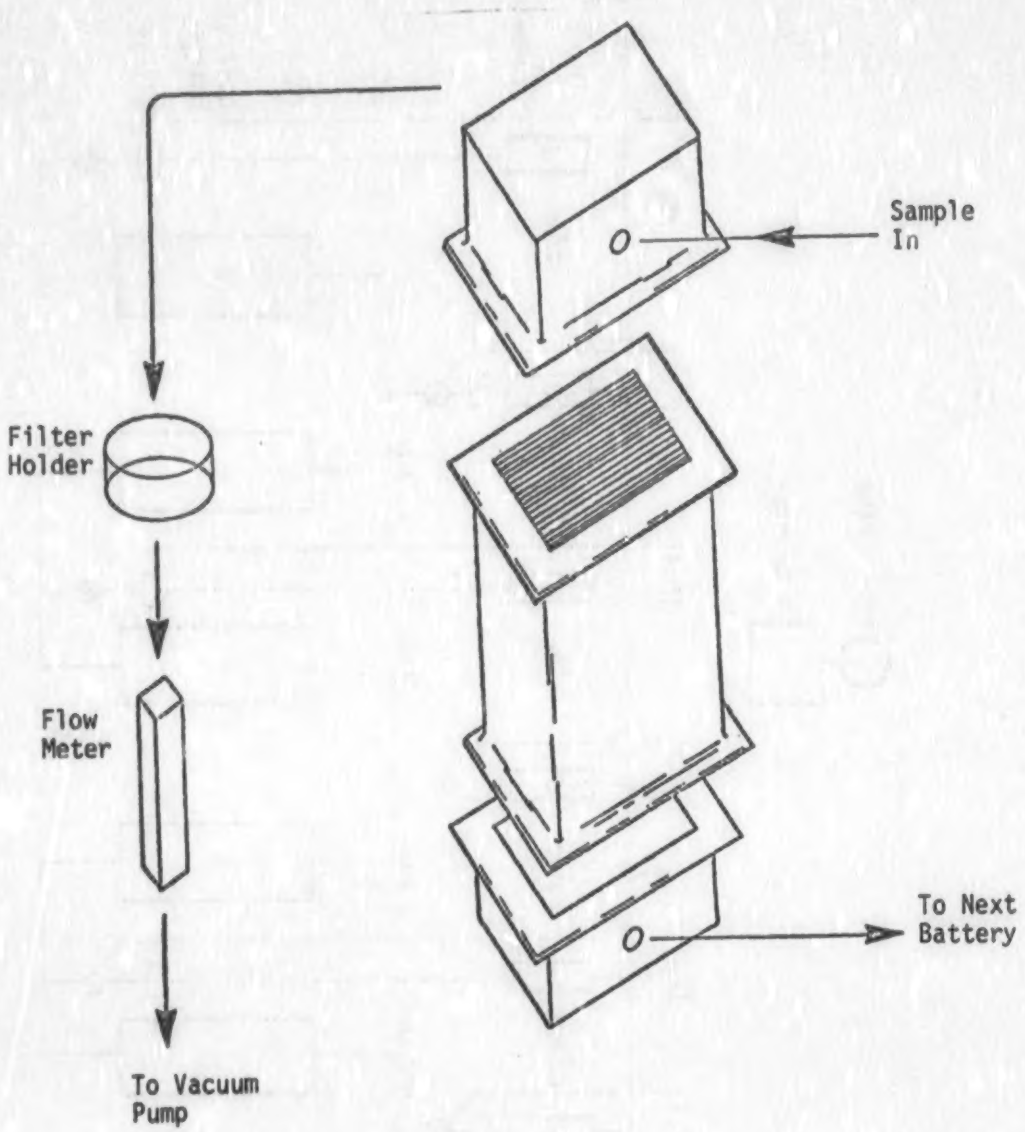
Sample In
Filter Holder
Flow Meter
To Next Battery
To Vacuum Pump

Figure 2. Single parallel plate diffusion battery.

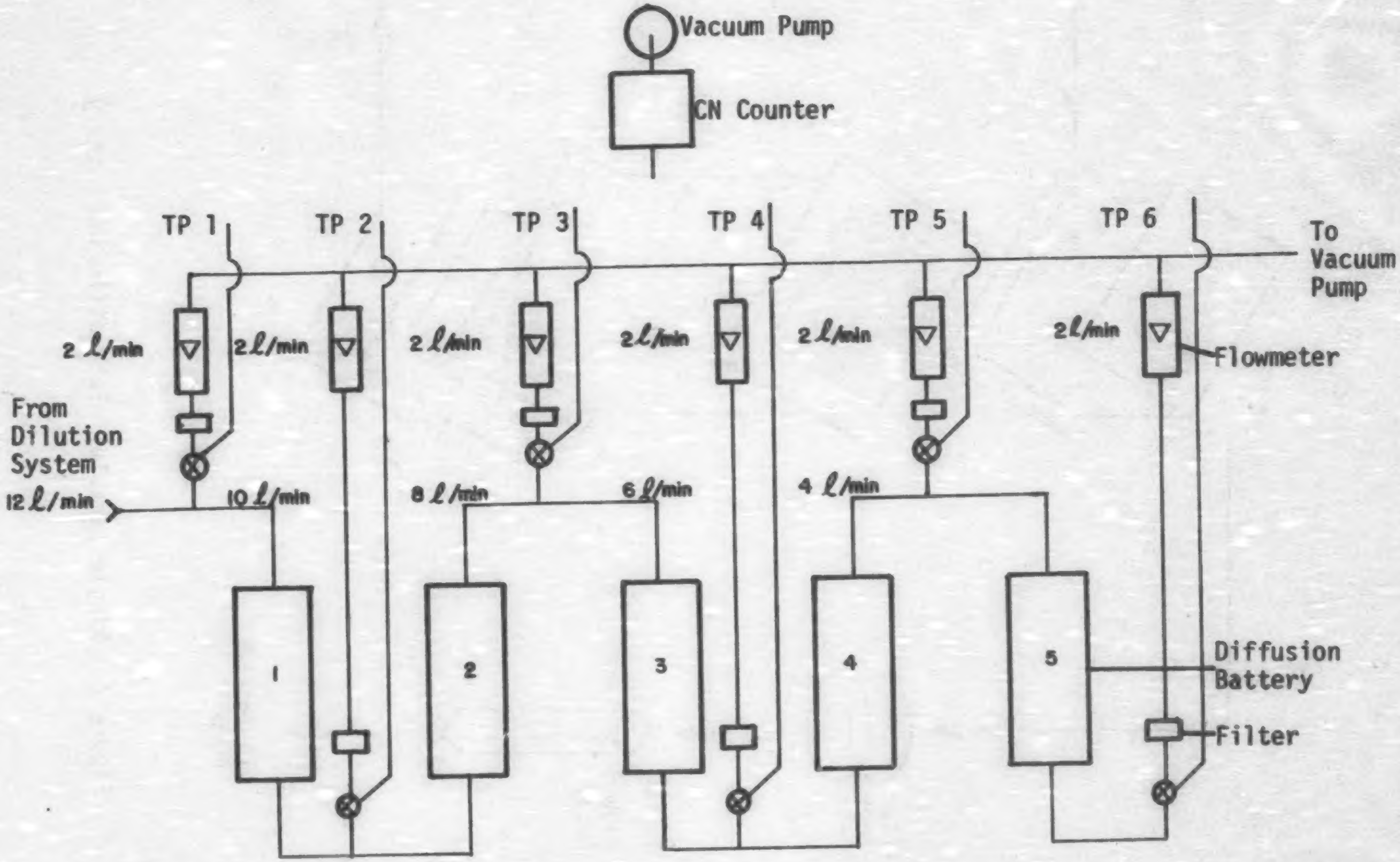
Vacuum Pump
CN Counter
TP 1
TP 2
TP 3
TP 4
TP 5
TP 6
To
Vacuum
Pump
2 ℓ/min
2 ℓ/min
2 ℓ/min
2 ℓ/min
2 ℓ/min
2 ℓ/min
Flowmeter
From
Dilution
System
12 ℓ/min
10 ℓ/min
8 ℓ/min
6 ℓ/min
4 ℓ/min
1
2
3
4
5
Diffusion
Battery
Filter

Figure 3. Diffusion battery system.

1. A condensation nuclei (CN) type detector, Gardner Assn., Inc.; and
2. A particle mobility analyzer, Model 3030 Thermo-Systems, Inc.

The CN type instrument gives total particle count by referencing the instrument readout with a factory supplied calibration curve. The mobility analyzer gives a particle distribution, and consequently total count, by referencing a factor supplied calibration factor to the digital electrometer current values displayed and continuously recorded. Furthermore, the mobility analyzer provides an opportunity to continuously observe the effects of process variations at both inlet and exit sampling sites. Finally, the greatest usage of the mobility analyzer is the quasi-continuous particle distribution information provided for the respective sample location.

The mobility analyzer is portable enough for near-simultaneous measurements at inlet and on sample locations. Care is exercised in transportation as well as sample preparation; the sample is required to be dry and electrically unbiased. Sheath air for the instrument is pulled through a bed of $CaSO_4$ desiccant to reduce moisture levels in the instrument. The output is recorded on a strip chart and subsequently uses factory determined calibration constants.

The filter samples are used to collect aerosol for microscopic inspection, chemical analysis, and to determine mass concentration. Particulate mass concentration can be used with number concentration to estimate particle density.

SUMMARY

The MRI fine particle extractive sampling system has been briefly reviewed. The laboratory and field data obtained with the system were in good agreement with cascade impactor tests and had good internal consistency between the various particle sensing methods. It is expected that these data will be subject to future research papers. Work is continuing in refinement of the equipment and test techniques.

ACKNOWLEDGMENT

The extractive sampling train and diffusion battery were developed with Meteorology Research, Inc., internal research funds.

PANEL DISCUSSION ON DIFFUSION BATTERIES

Dr. Seymour Calvert, Air Pollution Technology, Inc.

SPARKS: Seymour?

CALVERT: I haven't any slides for this. I wasn't prepared to talk about our diffusion batteries so I'll wave my hands around in the air a lot, maybe draw something on the blackboard if necessary, but the essentials are as Les Sparks alluded to. About 2 to 2-1/2 years ago, we got into the diffusion battery business and, upon hearing about the sad and strenuous adventures of Joe McCain and other people at Southern Research at carrying parallel plate batteries around, I decided that there ought to be a better way and came upon the idea of using screens, a series of screens, as the diffusional elements.

Contrary to a popular notion, there is a theoretical basis for screen type batteries. There are some uncertainties in the theory, but essentially you build it up from basically a description (mathematical model) of diffusional transfer to a single cyliner. Depending on whose theory you use there, there is some room for choice. Then the next point that comes up is that the cylinders are not isolated, but they are close together in really three dimensions by the time you put together a battery, so there are interaction effects. These have to be accounted for. We have put together a theoretical model and have calibrated our screen type diffusion battery using a parallel plate battery as the analytical instrument and have some pretty consistent results. We have used this in the field and have packaged it in aluminum suitcases, and we use a two-stage series dilution so that we can go up to a thousand-to-one without requiring very much dilution air. The aerosols that we have used for calibration purposes are spray-dried sodium chloride. We have also used some small polystryene latex particles, but these proved to be troublesome in experimental use.

We use condensation nuclei counters: a Gardner as the particle detector and counter. We also carry a Pollock type condensation nuclei counter along in the field and keep it in the lab or the motel room and calibrate the Gardner against it every night. We've found that this is necessary. We would like a better condensation nuclei counter or essentially a particle counter of some sort.

I think this just about does it. We are presently initiating a more comprehensive study program of the screen type diffusion battery, and we will be looking more thoroughly into such questions as transfer to single cylinders, interaction between cylinders, optimizing wire diameter mesh size, you name it.

DISCUSSION

_____: Characterizing a particle counter, what do you call a better particle counter?

CALVERT: We're using these manual condensation nuclei counters, and they have some bias, and there are some peculiarities in the way they count. You're able to set it at essentially different expansion ratios, which are supposed to give you different size ratios and they don't really work out the way that they are supposed to. It drifts from day to day. You have to keep the optics clean, carry extra bulbs, extra batteries, extra everythings as we've talked about, a little redundance doesn't hurt. The Pollack is just to big and heavy and beastly to carry around. So it would be nice to have a good continuous condensation nuclei counter, let's say about the size of the Environment I counter that SRI would feel was a good reliable instrument. I gather so far that you're not too happy with it.

MCCAIN: Only in part. It's lightweight and when it works it does all right, but I wouldn't use it as a primary instrument, the only instrument without something to back it up.

CALVERT: No. We used to use the G.E. CNC connected with preconditioning (chemical preconditioning) steps. Back at Penn State we were using it for gas analysis purposes and we were flying it , but we had to rent big airplanes.

BOLL: I seem to recall that at the Minnesota Symposium there were some good words said about Professor Picard's continuous CN counter , and I wonder if you or Joe McCain would like to comment on that.

CALVERT: I've no experience with it, but I've read some papers about that and then one by Sinclair using alcohol as the condensing medium, but I've no personal experience with it. Sounds like a great idea; it would be a good approach if it worked.

MCCAIN: Presuming it would work. We built one several years ago with the same sort of general idea using water vapor. We didn't achieve enough supersaturation in ours to reliably detect very small particles. The calibration would be the biggest problem to maintain a stable condition. I don't think that his is commercially available.

BOLL: No, it is not commercially available as far as I know.

MCCAIN: It would be a do-it-yourself thing. It might be a little expensive to build and get it to work.

HARRIS: The main thing is trying to get enough supersaturation to get it down to small things.

MCCAIN: It takes several hundred percent to do well on small sizes. We got a few percent with ours.

CALVERT: The one thing that I might say,thinking about it. The weight advantage of the screen type battery, size and weight advantage, we can get say 10 liters per minute sample capacity in something a foot long and about 1 inch diameter; 20 liters per minute in about an inch and a half diameter so we can handle the same sample, volumetric flow rate as a big parallel plate battery in a small screen setup. The basic difference being we don't have to have laminar flow through the casing. We can have either laminar or turbulent or mixed boundary layers on the wires, which is another story. Essentially, we can have pretty high superficial velocities through a unit. This proves to be a big advantage for field use.

_____: We found that the dilutions have to be extremely critical. What do you use to measure flow rates and what sort of dilution do you use, how do you dry your air? That sort of thing, Joe, too?

CALVERT: Yes, we went to the multistage dilution system because we didn't want to, among other things, we didn't want to have to meter a very small sample flow rate from the stack. You would like that flow rate to be as big as possible. If you want 10 cc per minute or some huge rate like that or a liter per minute, then if you want a thousand to one dilution on the other side, you now need a thousand liters per minute of filtered air, etc. It achieves two things: (1.) It keeps the meter size large (I don't like the idea of using a very small diameter orifice or Venturi, a restriction-type meter) and (2.) It keeps the amount of dilution air down. So we use glass Venturi meters for our sample measurement and for measurement of the sample for aerosol streams. For measurement of dilution air we use rotameters, the filtered dilution air.

_____: In your glass Venturi, the throat diameter is about what?

CALVERT: Let's see. Something on the order of 1/16", 1/16" to 1/8", or so.

MCCAIN: What we use are calibrated orifices of various sizes for the sample stream on the order of twenty-thousandths of an inch and up, depending on the flow rate we want.

_____: Do you have any trouble with edge effects or anything?

MCCAIN: There were some losses behind the orifice that were calibrated out, and there is some difficulty with metering with single stage dilution.

_____: What is the greatest you've ever had to dilute?

MCCAIN: We've probably run on the order of a couple thousand to one. We've gotten things that, orifices and the like, that at least calculate to provide up to 4,000 to 1 if necessary. More frequently, it will range from 10 to 1 to 500 to 1.

ENSOR: You can really affect your dilution by throttling the sample.

MCCAIN: We throttle the sample. We pull a sample at a high flow rate through a cyclone, through a majority of the probe, and then split off a small flow through the metering orifice to the diluter.

____: Do you get dilution that high?

CALVERT: We generally, I think we've run in the field up to about 100 to 1, or there about. Really, the dilution ratio required depends on the limits of the condensation nuclei counter. We were, as a matter of fact, trying to run the battery with the aerosol as is and do sometimes keep it, so we can run the aerosol through our diffusion battery without dilution and pull samples out at various points after so many sets of screens and dilute those. Dilute the sample and then run that through the CNC. So you can do it either way and we've run it with filters in after so many stages of screen and also had a bypass filter so we'd get a check on the total loading below the cut point of the impactor and precutter.

PANEL DISCUSSION ON DIFFUSION BATTERIES

Pedro Lilienfeld, GCA Corporation

SPARKS: Thank you, Seymour. Pedro?

LILIENFELD: The person who actually did the work in this area is not here, it is Doug Cooper, and so I'm not entirely up to all of the things that were done in this area, but I can relay a few of the conclusions and observations that were made in the use of both condensation nuclei counters and diffusion batteries in the field.

In one case, the problem of spurious generation of particles was also observed, that which Gil Sem reported this morning, measurements with the mobility analyzer where a large number of small particles was observed probably without a generation system being in the source itself, but somewhere between the source and the place of detection. It appears that that problem is very much associated with how the dilution is done and where it is done and at what temperatures it is done. So it all points to a condensation/evaporation problem somewhere between where the sample is taken before it reaches the place of measurement. We seem to have resolved that to a certain extent by building a diluter that fits right into the stack. By minimizing the length of line between the sample extraction point and where one dilutes it, it seems that the problem of spurious generation is minimized or at least reduced drastically.

Doug [Cooper] also worked on the problem of modeling the errors associated with the cut characteristics of diffusion batteries and comparing them with impactors. In his work, he came to the conclusion that small uncertainties in the characteristics of a diffusion battery can lead to very large errors in the measurement of size distributions using diffusion batteries, mainly because of the very gentle slope cuts of the diffusion batteries as compared to an impactor. One has to watch out very much for such errors in trying not to come to any sweeping wrong conclusions when such measurements are performed.

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Some problems with the condensation nuclei counter are found in the field. I'm just going to read some that he mentioned. The Environment I CNC which we have operates by measuring the light obscuration produced by the cloud, which results from an adiabatic expansion of the water-saturated sample taken about once per second from a continuous sampling flow of 30 to 60 cubic centimeters per second. This extinction is assumed to be linearly related to particle number concentration and independent of particle size for particles larger than 0.001 micrometers in diameter. Recent work on CNC's has somewhat tarnished their reputation. Cabro et al, have showed that the minimum detectable particle size is quite sensitive to relatively small changes in the adiabatic expansion ratios in the device and somewhat sensitive to typical changes in ambient temperature.

Walter Anjenica found that the supersaturations actually developed in the CNC were not as high as expected from the adiabatic expansion ratio, thus raising the minimum detectable particle size to 0.01 micrometers, somewhat dependent upon aerosol material tested.

Katz and Curshoned reported that even in the simple case of sodium chloride particles, the minimum detectable particle size was three times higher than theoretically predicted.

In general, it has been reported that nonwetting hydrophobic particles are not correctly counted. As for linearity of response, Lu and Pui, 1974. reported that the Environment I device, which is the one we were using in most of our tests, became nonlinear at concentrations above 50,000 per cubic centimeter, which is an order of magnitude less than its nominal upper limit of concentration. Lu and Pui also found out that the Environment I device that they tested was low by a constant factor of 2.5 in comparison with carefully determined concentrations in this linear arrangement. The General Electric model CNC was a factor of 1.4 too high. However, in many applications these errors are not significant as long as linearity is maintained.

From all of this one can conclude that the best way to use the CNC measurements is as ratios rather than absolute numbers. This should be limited to the linear response range. Furthermore, variations in temperature should be avoided, and aerosols of different material should not be compared directly. That is all that I have to contribute.

SPARKS: Thank you. Anybody else have any comments or experience with diffusion batteries, condensation nuclei counters? Or anything else they want to talk about?

We'd like to thank all of you for coming to the seminar and I don't know if we've solved any problems but we sure have discussed them.

GENERAL DISCUSSION - THURSDAY

SPARKS: This afternoon's speakers are really zipping right along. The next thing that is supposed to be on the agenda is a general discussion of everything that has happened so far. The floor is open if anybody has anything they want to say, if they have found something that might solve somebody's problems that they haven't mentioned or anything.

BOLL: I'd just like to take about a minute to remind you, I'm sure that you are all acquainted with the B&W Impactor that was designed and the operating procedure was published in December, 1971. I'd like to remind you that I think that has a few useable points that may still be valid. One of them is a cap over the inlet nozzle that would prevent anything from getting in through the nozzle while the impactor is warming up. Another is a critical flow orifice that was built into the impactor that allows you to start the flow rate up almost instantaneously and shut it down quickly so that you can sample very little, in 10 seconds if you like. Another is a sheath that covers the impactor and provides a thermally conductive path tending to minimize the possibility of thermal gradients from one to the other. They could get pretty horrible when you're sampling the high temperature duct and you've got part of the impactor hanging out of the cold end. That leaves you with the problem of deciding what is the gas temperature through the nozzles that are doing the impacting, and, therefore, what is the cut point? It does use a plain substrate which neither gains weight nor loses weight. Whether or not there is "bounce" is open to some debate, apparently. We have seen "bounce," but we judge the quality of the data by whether the cone of the impacted particles looks sound or whether there are any particles out on the edges of the cup. So I just brought up those points for your consideration when drawing up manuals and procedures.

HARRIS: I think those are very valid points, especially the problem of thermal gradients. Another thing, thinking about using these devices on pilot scale, we have to remember that these do have some finite volume to them, and if you try to jam a 3-inch Andersen Impactor into an 8-inch duct, you'll pretty well disturb the flow stream where you are trying to get a sample. The same can be said for most impactors. Although we say that you would like to sample in-situ, there are times when you're going to be stuck with using a probe. Have to make the best of the situation. We have, by the way, and I don't know the company right off-hand, we have purchased blankets which just wrap around the Brink Impactor for heating purposes. Rather than having to wrap the whole thing in heating tape, double-insulate and all that, you just buy these in any shape you want, and they fit nice and snugly and tie on. You can also get one to fit the Andersen, or something else. While I think about the heating problems, if you are going to use some kind of heat, I would like to recommend that you use your temperature-sensing element for controlling the temperature as the gas stream flow exiting from the impactor immediately after the impactor, rather than trying to use either the temperature of the surface of the unit or try to sense somewhere further on down the stream. It is amazing the amount of cooling that can happen in a gas flow stream in 5 inches. We first started off with just a little nipple, matter of fact, just 3 inches behind the Brink Impactors, and managed to barbeque the heating stage because the controller was always trying to drive the thing to a hotter temperature. It got up to 500°-600° and fried it, even though we were reading less than 300°F in the control circuits. So when you get into these regimes where you're having to heat, high heat, you have a lot of chance of creating more problems.

SPARKS: Everybody's happy, I guess.

SMITH: We don't have a lot of experience with those cyclones collecting gram quantities. Do you know from the TRW folks where the stuff ended up in the smaller cyclones? Did it work its way into the cup?

HARRIS: If I remember right, we had some evidence that the vortex was getting down into the collecting columns, because a lot of the material would be on the side of the collection cup. But I don't remember any large deposition found on the walls of the units. It seems to get down into the collection cup fairly readily.

SMITH: That's pretty consistent with what we saw on the calibration aerosols, but one flaw in our calibration procedures, I guess, is that we really didn't load them up with a lot of material. We just measured the collection efficiency with the ammonium flourascein and the Spectronic 38.

HARRIS: Well, when TRW took them out in the field, it was before we had them calibrated. We took them out, ran them, and tried to figure out what we had. They tried a very crude type of calibration: the type where you threw some dust in the air, sucked it through, then see where it ended up, then tried to do some SEM measurement on it. In one case, in that case, it is fairly heavily loaded and their results indicated that the last cyclone was less than half a micron,and they were getting down to the region where they were having some resolution problems. When we came up with individual particle calibrations, it was 0.3 microns, so there seems to be some sort of correlation involved there, but we haven't done it to the point where we know it was fortuitous or really true.

CALVERT: This is a prosaic item but an important one. Can we agree on what size the sampling ports ought to be? The standard is now 3 inches, but we're pushing for 4 inches,and when I was just thinking, feeling, that we were getting pretty porty, I started getting requests asking to put in 6 inch ports. I've seen these grow from 2 inches up to 3, which seemed pretty big at the time but now,with heating blankets around an impactor and other goodies, a 3 inch port isn't big enough and a 4 is just barely big enough and then if you have to turn the apparatus very much on an elbow and so forth, you'd like to have more. Yet, I think that it is important to have some kind of agreement on a standard approach so that when people are putting in sampling ports, they'll put in big enough ones so we can all be happy nationwide.

HARRIS: Arbitrarily, I say put in 6 inch.

CALVERT: I suppose we ought to think big.

HARRIS: It is kind of hard to ask them for a manhole, though.

CALVERT: You wouldn't want them to.

HARRIS: Then we would have ceiling problems.

CALVERT: No, you would have technician loss problems.

HARRIS: But one indication is the plant that we are going to next week is a fairly new power plant, and they have 6-inch ports throughout their system. They are also one of the few places that I know of where the sampling crew that worked for them got involved in designing the sampling ports. And they are nice to work with. If we have a choice, I'd like to see 6-inch ports. They are still manageable, but they give you a chance to try to do some different things rather than try to sneak it in and hope you don't scrape the side clean with your nozzle going in.

CALVERT: With flanges?

HARRIS: I believe that these happen to be nipples, I think nipples are what they need.

CALVERT: You could put a flange on, ok, nipples rather than unions for coupling thread. You could put a flange on.

HARRIS: Right, plus the fact that when you have the nipple there, that's the inside diameter which is what you are going to be dealing with, so you have a pretty good idea of what you need where, when its a coupling, and you have to worry about how far down is your nipple going to be and bring it on down.

ENSOR: With a cyclone sampling large amounts of flash, do you have problems with the deposit migrating during your tests, grating against themselves and creating new particles.

HARRIS: We don't know. I suspect that after next week, next couple of weeks, we'll have a pretty good idea of what is going on. The work that RTI did with the 1.0 CFM train, they had pretty good samples. Do you recall any problems they mentioned in collection, Bill [Kuykendal]?

KUYKENDAL: No, but I think that the collection was assumed to be where it was supposed to be.

HARRIS: I don't think that we ever saw any evidence that there was a patch of material, then it was clear, then another patch of material.

MCCAIN: Weren't some of those cyclones, on the abrasion question, I think that some of those were anodized aluminum, weren't they? Did you see any evidence of scouring through the anodized layer?

KUYKENDAL: On the outside?

MCCAIN: Inside opposite the inlet section, talking about abrasion.

KUYKENDAL: (To VanOsdell) You were there.

VANOSDELL: I never took one apart. I wasn't involved while the cyclones were being run. I remember that a great deal of the loading was on the first cyclone.

SMITH: With our calibrations the curve goes up to 100 percent and stays as opposed to the impactors, and you would hope that even for large quantities they would stay up, but it is not inconceivable that things would begin to be reentrained after you collect a lot on the walls.

HARRIS: Looks like we stuck our heads in the sand with you.

OLIN: I might mention that the ambient sampling people at EPA fooled around with the Aerotech tube and ran it at 40 CFM, that was about 6 years ago, and they thought that this was going to be the way to sample. That unit did break up floculates and give you a higher respirable mass than you normally should have. I think the reason there is it's a larger unit than is really pressed in terms of the sheer forces in there were pretty high. They ran at a cutoff of 3.5 microns and didn't have anything above it, for one thing, to take out real big particles. That's one thing that should be looked into.

HARRIS: Yes, unfortunately, the type of development that we've been involved with is that somebody has a problem, so we get something that works, then go back and see if it really does the job.

CALVERT: On your draft guidelines, the second on data reduction, I think the cut diameter approach is dealt with unfairly by comparing it with the deconvolution method on an aerosol with a very small standard deviation and the cut diameter method becomes better as the standard deviations gets bigger so if you're talking about a realistic geometric standard deviation then the cut diameter method is plenty good.

MCCAIN: It's even mentioned in that when you run real data with the deconvolution method it often tends to go to pieces. It requires large amounts of negative particles in some sizes on occasion to duplicate what your masses are on the stages of the impactor. It just doesn't work on every case.

ENSOR: We've had the same experience with the Picknett data reduction techniques. It will blow up for unknown reasons.

HARRIS: Yes, I think that what we're seeing is that, except for the very rare situation, we've gotten a perfectly adequate particle size description using D_{50}'s, rather than messing around with the Picknett.

CALVERT: I think it goes all the way back. The first paper that I remember on it is one by Mercer where he essentially validated that technique and indicated that it does depend on the sigma g.

HARRIS: Really, I think that it was introduced when we had those scalping cyclones, because some of them had some pretty lousy distributions, and some of the original scalpers did tend to have those tipped where they were extending over three size ranges, so that if you were assuming that it was all D_{50}, all of a sudden you didn't have enough material to work with down here in the second stage because it was in the cyclone. In that case, you had to do some subtracting in order to get a realistic distribution. I think your point is well taken. Those people that have had a chance to look at that thing and have some comments, please tell me, I would appreciate it. Tomorrow we're going to sit down and incorporate what everybody has said into the next edition of this. Then we'll be going out and will probably be recommending to our people who do tests for use that they use this. We'd like to get feedback now.

SPARKS: The one thing that all of our contracts are working on is that they will take blanks when they go out into the field. They will make blank samples, so that they will at least discover that there are problems with their substrates. I think they're all now, the technical directive for that was fairly recent, but I think they've all now had some experience trying to live with taking blanks in the field. I haven't heard any complaints, so I guess it's working reasonably well.

ENSOR: It's really no problem, because you can do blanks before the traverses. You're not worried about isokinetic sampling, so they require very little attention.

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